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AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

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THE

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I.—*On the Rotifer Conochilus volvox.*

By Henry Davis, F.R.M.S.

(*Read before the* Royal Microscopical Society, *June* 7, 1876.)

Plate CXLIII.

Few microscopic objects have been so much gazed at during the last dozen years as *Conochilus volvox;* scarcely a single scientific soirée, where the microscope has figured, has failed in a supply of their living crystal spheres; yet it would be difficult to mention a minute creature of any attractiveness which has been so little studied and is so little known. It is the object of the present paper, in part, to supply the omission, and to piece together, in a manner as disjointed as may be, the notes made on this beautiful rotifer, at somewhat wide intervals during the last seven years.

My early gatherings were made at Hampstead Heath in 1868, but lately this locality has failed me, and by the assistance of my friend, Mr. F. Oxley (who is the best collector I know), a fairly continuous supply has been kept up, mainly from the ponds about Snaresbrook. Now a careful scrutiny of the very first gathering revealed the fact that, judged by the rules of symmetry as applied to the Melicertans (among which, latter-day observers class Conochilus), this rotifer has its leading features entirely dislocated. The most striking peculiarities are found in and about the head; but before going into particulars, it may be well to note what authority says about it. Ehrenberg tells us: "The frontal region

EXPLANATION OF PLATE CXLIII.

Fig. 1.—Diagram sketch of head of a Melicertan (Limnias).
A. Aperture of mouth outside the disk.
„ 2.—Diagram sketch of head of female Conochilus.
A. Aperture of mouth inside the disk.
„ 3.—Group of *Conochilus volvox* (female), × about 300.
„ 4.—Male „ (male) „ „
„ 5.—Egg of male „ .. .. „ „
„ 6.— „ female „ .. .. „ „
„ 7.—Ephippial egg „ .. .. „ 750.

of the animalcule is broad, truncated, and surrounded with a wreath of cilia, interrupted at the mouth which is lateral. On the frontal plane arise four thick conical papillæ, often furnished with an articulated bristle, especially the two anterior." Gosse says: "The frontal disk is large, and divided into two semicircular portions, round which the cilia seem to be set; yet, when they are in active rotation, the eye cannot discern any break in the ciliary crown. The centre of the front rises into a blunt cone, on one side of which projects a little jointed antenna, bearing a bristle at its tip."

Mr. Gosse alludes to the discrepance between his account of the rotifer and Ehrenberg's, and notes the possibility of their being different species; but I am constrained to believe from very patent errors in Mr. Gosse's observation—to be noticed farther on—that both naturalists examined the same species and both made dissimilar mistakes. Of course Ehrenberg's wonderful work is beyond detraction but just within criticism; and if Mr. Gosse make a slip or two, these are readily condoned in the remembrance of his many discoveries and the exceedingly clever papers he has written for us. In the case of his article on Conochilus * there is internal evidence that its study was taken up for a short time very early in his microscopical career, that sketches and notes were made at the time from which *some years after* the paper was elaborated. Had the observations been made by the light of his later experience, the result had been very different, and probably this paper not written. Moreover, Mr. Gosse never had the immense assistance of Mr. Wenham, who supplies modern naturalists with an instrument making minute investigations as easy as reading with spectacles.

But we neglect the rotifer. The facts are that the part where the "wreath of cilia is interrupted" is *not* the mouth; there are *not* "four conical papillæ each with a bristle," but only two short contiguous antennæ ("calcars" of Ehrenberg) furnished with pencils of fine long setæ, and the true oral aperture is a slit on the dorsal side of the rounded cone rising from the centre of the trochal disk.

The peculiarity of this arrangement of features will be seen by comparison with those of an undoubted Melicertan. Fig. 1, Pl. CXLIII., represents the upper part of a Limnias; the ciliated disk is turned up and away from the observer, bringing the mouth aperture into view at A. Just as described by Huxley as existing in Lacinularia, a collar of fine cilia is seen running round *outside and beneath* the larger cilia of the disk, and this collar leads directly to the mouth. Food can often be seen traversing the channel between the coarse and fine cilia, and rapidly becoming engulfed in the buccal funnel. Now turning to Fig. 2 we see the mouth aperture *on and above* the disk. The finer ciliated frill is here *within* the bolder coarser one. Further, in the Melicertan (Fig. 1)

* 'Pop. Science Review,' vol. i. p. 491.

we have the small antennæ widely separated, one on either side of the body; in Conochilus (Fig. 2) these are close together and placed on the crown of the head.

The slit of the mouth being towards the dorsal side makes another departure from the Melicertan type; but it is worthy of notice that the "set" of the mastax ("gizzard"), with its lower narrow ends pointing to the ventral side, also the position of the ovary and other internal parts, are precisely as if the oral aperture were opposite to its true position, and quite favours the fancy that Conochilus was once a Melicertan; that the circumstance of mutual agglomeration had necessitated the displacement of the opening to the mouth, which had drawn through the usual notch in the ventral edge of the disk (still traceable), carrying the fine ciliated collar and antennæ with it well over to the middle of the disk.

The obscure division of the alimentary canal into three parts, as seen by Huxley in Lacinularia, is very plainly marked in Conochilus, especially when they are filled with differently coloured food, or with carmine, indigo, &c. The contents of each division does not appear to filter gradually from one to another, but empties suddenly, thus:—the contents of the rectum being discharged in one pellet, after a few seconds it is replaced by the full charge of the central compartment of the alimentary canal, and this again by all in the stomach, which at once begins to fill up from the food passing from the mastax.

The usual "tremulous tags"—four pairs—I see in Conochilus, but no contractile vesicle. The eyes are very fine; each a clear colourless globe imbedded in a red pigment—they seem fixedly directed upward and outward.

The more we examine and understand the rotifers, the farther we seem from a basis on which to found a satisfactory scheme of classification. What we might well consider the best as applied to the "builder" rotifers by Gosse breaks down entirely with Conochilus. It cannot possibly be ranked with Megalotrocha (itself plainly a Lacinularia), and is but distantly related to his family Melicertadæ. It comes half-way between the Floscules and the Melicertans, having the oral aperture situated and armed like that of the former, but with the active ciliated disk of the latter.

Surrounded by the clear jelly in which the little animals live may be found in nearly every well-grown colony two distinct kinds of eggs; more rarely a third smaller sort is seen (male egg, Fig. 5). The female egg is transparent, nearly colourless, and, when far developed, the fœtus with its large mastax may be plainly seen within (Fig. 5). The smaller egg is equally clear, and reveals the male very neatly packed up; but, even at this tender age, restless and gymnastic beyond belief. The other kind of egg ("ephippial" of Huxley) is somewhat larger than the ordinary female egg, of

little colour by reflected light, but nearly opaque, and reticulated with dark lines well within the shell (Fig. 7 greatly magnified). Wherever the dark lines appear to cross there is a projection like a short tube, which is directed outwards to the shell, but does not quite reach it, being stopped probably by an inner membrane. It bears also a dark mark like a suture all round the egg at about one-third from the end. That the term "winter egg" applied often to this form is a misnomer there can scarcely be a doubt, for I have found them in every month in the year; but my belief is (as has been suspected of the ephippial eggs of the Entomostraca) that these eggs are destined to preserve the species through the drought to which the ponds, that the animals flourish in, are constantly liable. On a pond drying up, the clusters of rotifers, holding the ephippial eggs, sink to the bottom, are dried in the mud, and development probably suspended, only to be renewed in the next rainy season.

On breaking one of the ephippial eggs it appeared to contain only extremely small granules, from which fact it may be supposed to be very slightly developed when first deposited. Trying to imitate the conditions existing in their native pond, I saved some of these eggs in their surrounding clear slime, allowed them to dry spontaneously in a small glass tank, kept them dry for a month, and have since supplied them with water during some ten months, occasionally looking at them with a low power. But one cannot stare continuously for many months even at a rotifer, and during this time some have mysteriously disappeared, and some remain, little altered, except that they are more transparent, the markings grown fainter, and altogether more resemble the ordinary eggs. Thus the evidence of this experiment is all negative, but equally so are the results of the pond from which the original colonies were taken: it dried up, with myriads of ephippial eggs in it, in September 1875, again filled later on, but up to the end of last May yielded not a single group of Conochilus. Still there they are, and sooner or later must show themselves: each ephippial egg, I believe, will hatch out; the young rotifer being very close to another (brought down with it in the gelatinous sphere holding many and gluing all together), will "swarm" with its neighbours (*à la* Lacinularia), and—behold a young group of Conochilus! As few as six may be seen in a group, and these all apparently of one age; but when the clusters contain many individuals, two or more generations may be generally recognized.

The clear slimy secretion, more or less common to all rotifers, and so variously used by each species, plays a very important part in the economy of Conochilus, and one hitherto unsuspected; the creatures are imbedded in it very closely as to their foot-tails, but more and more loosely in approaching the heads; their extremities

are *not* attached to each other, as affirmed by Gosse* and others, but merely approximate, and a curious result is a consequence, which could never occur did they hold each other;—as the young female rotifers hatch they squeeze each a place for herself, and increase the constant drag in every direction which is exerted on the gelatinous ball holding them altogether, but when the inhabitants have greatly increased—beyond ninety or so—the strain is too great, the mass gives way at the weakest place, there is perfect subdivision and *two* rotating clusters; these two clusters again increase, again subdivide, and so on until the pond is alive with them.

The subdivision of Conochilus clusters might have been expected had anyone thought of it; but observation only led me to it: every large group seems in a state of tottering equilibrium, and a slight touch splits it up, while the resilience of the gelatinous mass and the motion of the rotifers quickly round the angles of each hemisphere. I have seen one group divide into three, and can generally coax a large cluster to divide neatly by a quick gentle squeeze in a live-box; even under these trying circumstances the rotifers continue to preserve their radiate habits.

I have only to add a few remarks on the male; but first am obliged to notice another inaccuracy of Mr. Gosse's: he says, in the article already cited, "the form of the male egg is very peculiar; it appears to be nearly circular, flattened on one side and convex on the other; there is considerable difference in their size; they are of a pale yellow hue, marked with several blackish specks. . . . I did not trace the development of any of the eggs." Now those flat, yellow, speckled eggs are nothing more or less than the discharged fæcal pellets! Mr. Gosse's description and figure fits them exactly, while we have seen that the true male (whose development *has* been traced) are clear, colourless and oval.

The male of Conochilus I saw for the first time—although long expecting him—in the summer of last year, and beyond his rarity saw little in him to admire. He is scarcely larger than the head of one of his sisters, and resembles some of Mr. Gosse's figures of other male rotifers, and most of Dr. Hudson's, except that I could not detect any antennæ on the back. His constant endeavour while under the microscope is to prove himself without form and void, to simulate a preternaturally lively Amœba, and, as soon as possible, to wriggle himself *first* to decomposition, then to death.

* "They adhere together by the mutual contact of the bases of their feet . . . . they are united by the extremity of the foot."—Gosse, 'Pop. Science Review,' August 1862.

II.—*On the Abbé Count Castracane's Photographs of Nobert's 19th Band.* By H. C. Sorby, F.R.S.

I have lately received from Count Castracane some photographs, taken by him, of Nobert's 19th band, and a letter on the subject, which I translate as follows:

"I read with much attention the interesting address given by you at the annual meeting of the Royal Microscopical Society, and having well considered it, I wish to be allowed to make some observations on the ultimate limits of the microscope, referring to some personal experiments, which, however they may on the whole tend to confirm what you have demonstrated, yet still seem to prove that the formula of Helmholtz is not yet the last word that may be said on the subject of the final powers of the microscope.

"The illustrious American microscopist, Dr. Woodward, having had the politeness to send to me examples of his magnificent microscopical photographs, and amongst these those of Nobert's lines, I also wished to ascertain whether I could succeed in photographing the 19th band, containing a series of lines at a distance of $\frac{1}{112320}$ of an English inch. After many and various trials, I was at length most fortunate in reproducing those exceedingly fine striæ, by using a magnifying power of 800 linear. The photographic negative was, however, much obscured by general shading, but as it was more in accordance with what could be seen by direct vision, I was satisfied with the result. In order to convince myself that the lines seen on the photograph are genuine, and not due to interference fringes, I resolved to measure their distance by means of the microscope, and by dividing this distance by the magnifying power, I found that I had not been deceived. That I did not find the thirty-seven lines observed by Dr. Woodward, depends on the difference in the preparation of the original. I therefore have also obtained the resolution of a series of lines at a distance of $\frac{1}{112320}$ of an inch, by using a photo-micrographic objective of $\frac{1}{13}$ of a German inch in focal length, very accurately constructed for me by Gundlach.

"In your address you asserted, on the authority of Helmholtz, that the limit of the power of resolution of a dry lens was equal to three-quarters that of an immersion lens, all the conditions being the same, and thus if I had used an equally perfect immersion lens I may perhaps conclude that I should have arrived at a resolution of $\frac{1}{140000}$ of an inch. At all events, it appears that I may conclude that with a dry lens I have attained to the true and certain resolution of $\frac{1}{112300}$ of an inch, which is somewhat beyond the limit indicated by Helmholtz's theory.

"Although I do not feel able to demonstrate theoretically how this is possible, I am anxious to furnish the data necessary to enable

others to do so. I will therefore state that the photograph was obtained by means of a dry lens by Gundlach, of $\frac{1}{13}$ German inch focal length, and the object was illuminated by a triple condenser of very wide aperture, made for me by Nachet, of Paris, used with a stop which allowed only a small but very bright concentric beam of light to fall on the object at a very oblique angle.

"In the hope that an account of the result which I have thus obtained may serve to make more complete the theory of the ultimate limit of vision with the microscope, and anxious to obtain the opinion of those competent to judge in such a question, I have the honour to remain

"Your obedient servant,

"F. Castracane."

With reference to the facts here described by Count Castracane, I wish to offer a few remarks. It appears to me that the visibility of the fine lines of Nobert's test-plates depends on several different circumstances. The light must be thrown in such a manner as to be definitely intercepted by the marking on the glass, or they could not possibly be seen; and we have then to consider the effect of interference fringes, as well as the quality of the microscope itself. I do not see that there ought to be any serious difficulty in explaining on Helmholtz's principles the resolution of Nobert's 19th band. With such an illumination as that adopted by Count Castracane, it appears to me very probable that the interference fringes would so far coincide with the true lines as not to prevent a satisfactory definition. At the same time I am anxious to make it fully understood that in my address I endeavoured more to point out the results that would follow from Helmholtz's theory, than to examine whether it is or is not in every respect true. I should be one of the last to wish it to be looked upon as a final solution of the problem. I think many questions remain to be cleared up by the actual observations of persons conversant with the theory, and accustomed to the practical use of high powers. I am also inclined to believe that several crucial tests ought to be examined. Amongst these I would especially suggest the study of fine lines at very close yet *unequal* intervals, and of lines at equal intervals with one or two *missed out* here and there. Theory indicates that such tests would be far more difficult to see correctly than lines ruled at regular and equal intervals; and an examination of such tests ought to afford much information respecting both the final powers of our microscopes and the physical constitution of light itself. Helmholtz of course assumes the truth of the undulatory theory; and though in the highest degree probable, it would perhaps be premature to conclude that it is absolutely certain, when applied to the explanation of every

phenomenon, especially in such a case as that under consideration. I am very glad that what I said in my address has been the means of drawing attention to this question, since we cannot expect to arrive at perfectly satisfactory conclusions, unless the subject is examined by various observers, using independent means of research.

III.—*On the Aperture of Object-glasses.* By F. H. Wenham.

The slit that I have employed for cutting off the lateral rays of an object-glass (which indicate an erroneous aperture beyond the true angle), consisted of a clean line cut through a thin coating of black varnish. As it is a difficult operation to lay this on uniformly, and it has besides an objectionable thickness, I now make use of a slit constructed as follows. A 1×3 slip of glass is moved about over the flame of an ordinary petroleum lamp, till the black deposit nearly ceases to be transparent. This coating has but little coherence, and particles are swept before a scribing point, so that a clean cut line cannot be made; but if a drop of turpentine is caused to flow gently over the smoke deposit and then evaporated by heat, the film has some consistence, and a narrow clean cut slit can be made through it, with the keen point of a penknife drawn along a straight edge; a thin glass cover is now laid over the slit, and Canada balsam run beneath it by capillary attraction aided by heat.

A slit prepared this way has some advantages. It is protected from dust and injury. It enables the object-glass to be tested for either dry or immersion under the proper adjustment in either case, for the usual cover thickness. This adjustment can be easily made on the edges of the slit itself or any detached particle. Finally, the rays of the pencil are refracted outwards to a less angle through a slit mounted in balsam, which consequently may be narrower than an air slit, without the risk of interfering with very oblique rays.

I am quoted in the last Journal* as having many years ago affirmed that Professor Robinson's method of measuring apertures is by far the best. It makes no difference whether Professor Stokes "admits the validity of this" or not; the question must be decided by fact, and not by force of opinion. If everyone else has hitherto been wrong, so have I; and I cannot admit the concluding sentence in Mr. Hogg's review in the last Journal, that "with Professor Robinson's modified method no slit is required, and most conclusive and reliable results will be obtained;" for the

* Page 266.

lateral pencils directing light far beyond the axial one greatly enlarge the diameter of the proper light disk, and an excess or false aperture is delineated. My present standpoint is, that every method, *without exception*, hitherto employed in measuring angles of aperture is exceedingly erroneous. An aperture mapped out on a screen shows very instructively the outline of the false aperture and the true one, as obtained with the slit. The first is faintly portrayed as an outer circle of light while the bright disk given by the slit takes an oval form within the other. This is more difficult to manage than the slit with the ordinary sector method and lamp, by which it is easy to focus and adjust for the thickness of cover.

I give the result on three object-glasses made nearly twenty years ago, viz. a $\frac{1}{5}$th, an $\frac{1}{8}$th, and a $\frac{1}{12}$th, whose apertures were stated to be 100°, 130°, and 170°; these measured with the slit gave 56°, 92°, and 100°.

I invite fair discussion on the question; as one of science, it should cause no feeling of the animosity displayed by a few whose only motive appears to have been to endeavour to show me in the wrong.

IV.—*Embryology of Salpa.* By W. K. Brooks, Ph.D.

Plate CXLIV.

Students of the embryology of the various forms of Tunicata are so numerous and active at present, that the naturalist who refrains from publishing any new facts which he may acquire until the figures necessary for their illustration can be prepared, is very apt to find that they are no longer new. The following brief abstract of the more important points in the history of the development of Salpa has therefore been drawn up, as the precursor of a more extended description which is now in preparation.

At the time when the Salpa-chain escapes from the body of the solitary form, each individual of the chain contains one ovum, which is enclosed within a capsule of epithelial cells, and is suspended in the sinus system of the "zooid" on the neural side, between the stomach and the atrial orifice, by means of a gubernaculum, by which it is attached to the wall of the branchial sac. (See Fig. I.)

The ovum shows no trace of a vitelline membrane; the yolk is composed of transparent protoplasm without granules, and the germinal vesicle contains no dot, but seems to be homogeneous.

Impregnation takes place through the action of the spermatic filaments which are discharged into the water by the zooids of

other full-grown chains, are drawn into the branchial sacs of the immature zooids which contain the eggs, and penetrate into the interior of the gubernaculum.

Upon impregnation the germinative vesicle disappears; the gubernaculum becomes irregularly swollen and shortened, thus drawing the egg down into the brood-sac, which is formed by an involution of the branchial sac of the nurse (Fig. II.). The egg, nourished by the blood which bathes it, rapidly increases in size, and undergoes a process of *total* segmentation, as the result of which two portions are formed; a finely segmented "germ yolk," and a less completely segmented "food yolk." (Fig. V.)

The latter becomes enveloped by the former through a process of invagination, forming a true "gastrula" or "invaginate planula," the opening of which, the "orifice of Rusconi," persists and forms the orifice of the placenta. (Figs. VI., VII., VIII., *f.*)

The embryo, still growing rapidly, becomes divided into two portions by a constriction (Fig. VII.); the portion nearest the point of attachment to the brood-sac forms the embryo proper, and the remaining portion that part of the placenta which is to be in communication with the sinus system of the fœtus. (Fig. VII.)

Within this portion there is a cup-shaped cavity, part of the original "cavity of Rusconi," which is in direct communication with the sinus system of the nurse, and thus forms the second or inner chamber of the placenta. This soon becomes divided up into a great number of irregular intercommunicating lacunæ, which are produced by the growth of a structure resembling a stump with its roots, and which seems to be formed directly from the blood of the nurse, by the aggregation and fusion of the blood-corpuscles.

The subsequent development of the fœtus, which is the young of the solitary Salpa, is substantially as it has been described by Sars, Krohn, Vogt, Huxley, Leuckart, and others, and I have been able to add little to what is known upon the subject.

The atrium of Salpa has been supposed to lack those lateral portions which, in most Tunicates, lie upon the sides of the branchial sac and are called the lateral atria; but at an early stage these seem to be present, as well as the mid-atrium, but the cavities of the lateral atria never become connected with that of the branchial sac by the formation of branchial slits; and at a very early period of development the walls of each lateral atrium unite, thus obliterating the cavity, and giving rise to a broad layer of tissue upon each side of the body, between the branchial sac and the so-called "muscular tunic," the "outer tunic" of Huxley.* Rows of transverse slits soon appear in these layers, which thus become divided to form the muscular bands, which latter subsequently

* This "outer tunic" must not be confounded with the "cellulose test" of Huxley, which covers it.

become united to the inner surface of the outer tunic. (Fig. VIII., *m.*)

The sides of the mid-atrium become united at two points, one on each side, with the posterior surface of the branchial sac, and as the atrial and branchial tunics are free from each other between these regions of union, a median longitudinal sinus is thus formed which is the "gill" or "hypopharyngeal band." The central portions of the two regions where the tunics are united, are soon absorbed, and a single branchial slit is thus formed on each side of the gill.

The earliest stages in the formation of the atrial chamber were not observed, but nothing was seen which seemed to indicate that it is formed, as in most Tunicates, by tubular invaginations of the outer wall of the embryo.

The cavity of the œsophagus is a prolongation of that of the branchial sac, and was in direct communication with this at the mouth when first observed. The stomach is formed as a diverticulum from the side of the œsophagus, and the cavities of the two were connected at all the periods observed, but the cavity of the intestine originates independently, and at first is closed at both ends; the partition between it and the stomach disappears first; that at the anal or atrial end persists some time longer.

The few facts which I have been able to add to what is known of the development of the Salpa-chain relate, for the most part, to the earliest stages in the development of this, which has always been considered the sexual generation; and seem to prove that the solitary Salpa is the female, and the chain Salpa simply the male, which does not reproduce, but merely serves to fertilize and nourish the egg, so that we have, not an alternation of generations, but a very remarkable difference in the form and mode of origin of the two sexes.

The tube or stolon which is to form the chain first appears as a protrusion of diverticulum from the outer or muscular tunic of the solitary Salpa, directly opposite the heart; this protrusion rapidly increases in length, and soon presents the form of a long tube closed at its distal end, projecting into the test, and with its cavity in direct connection with the cavity of the sinus system (the body cavity) of the solitary Salpa, so that the blood of the latter enters and circulates freely within it. (Fig. X.)

A second tube with very thick walls and a very narrow cavity now grows out from the pericardium, crosses the sinus and penetrates the cavity of the outer tube almost to its tip or blind end, and soon becomes flattened and its edges unite with the walls of the outer tube, which thus becomes divided into two chambers, which are entirely separate from each other except at the tip. The blood now passes into one of these chambers at its base, and is

driven up to the blind end where it passes around the partition, back through the other chamber to the sinus of the parent. It is of course unnecessary to state that when the circulation of the parent is reversed that of the stolon changes also.

By the formation of the partition above described the tube is divided longitudinally into halves, and each half is destined to be converted into the series of zooids on one side of the chain. The outer wall of the tube, which has been shown to be part of the muscular tunic of the parent, becomes the muscular tunics of the zooids; the chambers, which are continuous with the sinus system of the parent, form the body cavities or sinus systems of the zooids, and the central tube, which is a prolongation of the pericardium of the parent, forms the nervous, digestive, and branchial organs of the zooids of the chain. It is probable that the cavity of this inner tube gives rise to lateral diverticula, which form the cavities of the digestive organs and branchial sac of the young, but this point could not be determined with certainty, nor could any connection between the cavity of this inner tube and any of the cavities of the parent be discovered.

Before the tube becomes differentiated into the organs of the zooids, in fact, before there are any indications that the tube is to give rise to the chain, two new organs are formed, one in each of the sinus chambers of the stolon. These new organs are long club-shaped masses of protoplasm, which are not at first attached to the tube, but are free within the chambers, and do not seem to be derived from any of the pre-existing parts of the solitary Salpa, but are formed directly from the blood. As the tube grows these organs lengthen as well, and soon a row of germinative vesicles is seen extending along each of them; they are the ovaries. (Fig. X., *x.*) At the time that the constrictions, which are the first indications of the zooids, make their appearance on the outer wall of the tube, each ovary is seen to be made up of a single row of eggs, equal in number to the constrictions which indicate the number of the future zooids, and as these latter are developed, and their sinus systems become separated from the common cavity of the tube, the chain of ova divides, so that a single egg passes into the sinus system of each zooid, and becomes suspended there by a gubernaculum, by means of which it is attached to the wall of the branchial sac, as already described.

Since the chain Salpa at birth always contains an unimpregnated ovum, organically connected with its body, and since this egg and the resulting embryo are nourished by the blood of the chain Salpa by means of a placenta, and since no reproductive organs have ever been observed within the body of the solitary Salpa, it seems most reasonable to accept the belief that the solitary Salpa is the asexual, and the chain Salpa the hermaphrodite sexual generation, and that

the developmental history of the genus presents a true example of "alternation of generations." When, however, we have traced backward the history of one of the zooids, which compose a chain, and find that the egg is present at all stages of growth, and is of exactly the same size and appearance as at the time of its impregnation; when we find one organ after another disappearing, until at last we have nothing but a faint trace of a constriction indicating upon the wall of the stolon the position of the future zooid, the conclusion seems to be irresistible that the animal, which has as yet no existence, cannot be the parent of the egg which is already fully formed.

The life history of Salpa may then be stated in outline as follows: The solitary Salpa is the female, and produces a chain of males by budding, and discharges an egg in the body of each of these before birth. These eggs are impregnated while the zooids of the chain are very small and sexually immature, and develop into females which give rise to other males in the same way.

After the fœtus has been discharged from the body of the male the latter attains its full size, becomes sexually mature, and discharges its spermatic fluid into the water to gain access to the eggs carried by other immature chains.

The fact that impregnation takes place, not, as we might expect, within the body of the solitary, but within that of the chain Salpa, is no objection to this view, for the number of animals whose eggs are fertilized within the body of the female is quite small, and in at least one genus, Hippocampus, the eggs are received into a specialized brood-sac in the male, and are there impregnated.

We can also find analogy for the singular fact that the eggs always develop females, while the males are formed by budding. The fertilized eggs of the bee always give rise to females, while the males are developed by the virgin bee, through what seems, as pointed out by Professor McCrady, to be most properly regarded as a process of internal gemmation; and we cannot fail to mark the very striking parallelism between the process of reproduction as manifested in Salpa and the bee.

The fertilization of the eggs within the bodies of zooids produced by budding from the body of that whose ovary gave rise to the eggs is not unusual among the Tunicata. The zooids of most of the Tunicata are hermaphrodite, and develop eggs of their own, but, at least in the case of Pyrosoma, Perophora, Didemnium, and Amauricium, the egg which undergoes impregnation and development within the body of the zooid is derived, not from its own ovary, but from that of the generation before, and the eggs produced in the body of the second generation must pass into the bodies of the zooids of the third generation before they can be fertilized. The essential difference between this process and that presented by Salpa, is that in

Salpa the sexes are distinct, and as the chain Salpa has no ovary the process of budding stops with the second generation; while as the zooids of the other Tunicata are hermaphrodite the process may go on indefinitely.

The history of Salpa is of especial interest, as it throws a great deal of light upon the manner in which separation of the sexes may be brought about in forms which were originally hermaphrodite, and it is also interesting to note that the elæoblast, the history of the development of which shows it to be the homologue in the female of the testicle of the male, is concerned in reproduction, although it has lost all the characteristics of a sexual organ, and is simply a supply of food.

We cannot fail to notice the connection between the manner in which the male Salpa is produced, and the numerous cases, through the various groups of the animal kingdom, in which the male is, to some extent, parasitic upon, or supplemental to, the female.

The Cirrhipeds, Arachnids and the Argonaut, will at once suggest themselves, as familiar instances of the occurrence of such a relation between the sexes.

These interesting theoretical points are simply mentioned here, as a more exhaustive discussion of them is reserved for another place.—*A Paper read before the Boston Society of Natural History.*

EXPLANATION OF PLATE CXLIV.

The small letters have the same signification throughout.

a. Wall of branchial sac.
b. Wall of outer tunic.
c. Sinus cavity.
d. Branchial cavity.
e. Egg.
f. Opening of inner chamber of placenta.
g. Cavity of inner chamber of placenta.
h. Cavity of outer chamber of placenta.
i. Branchial aperture.
k. Atrial aperture.
l. Cavity of atrial chamber.
m. Muscles.
n. Ganglion.
o. Nucleus.
p. Œsophagus.
s. Stomach.
t. Intestines.
u. Elæoblast.
v. Pericardium.
w. Inner tube of stolon.
x. Ovary.

Fig. I.—Egg within the sinus system, and attached by a gubernaculum to wall of branchial sac, within the cavity of which a few spermatic filaments are seen.

Figs. II., III., IV., and V.—Successive stages of segmentation.

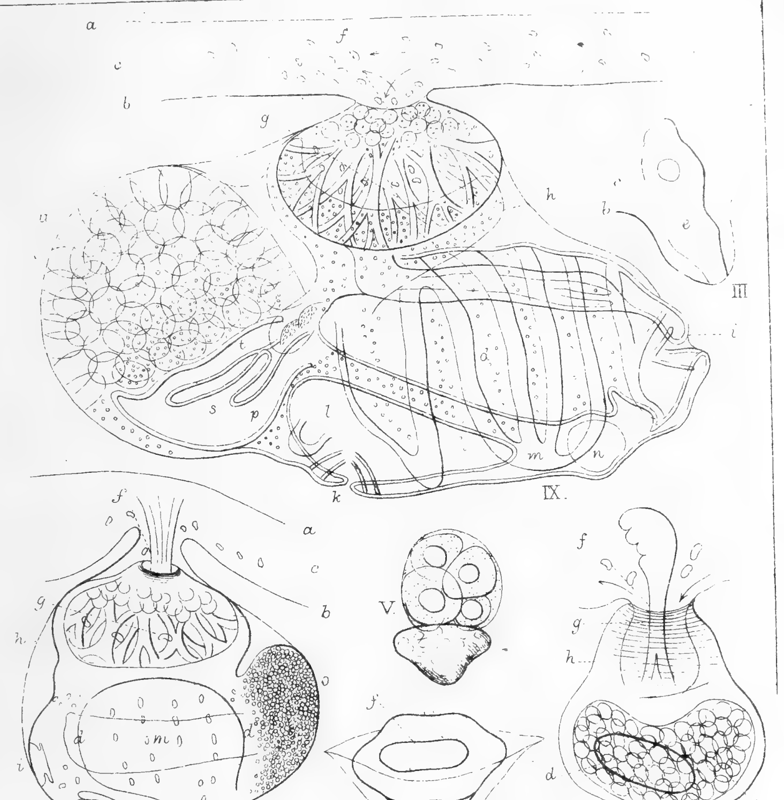
Fig. VI.—Gastrula within the brood-sac.

Fig. VII.—Embryo, soon after the primitive digestive cavity has become divided into the branchial and placental chambers.

Fig. VIII.—Embryo considerably advanced, showing the mid-atrium *l*, and one of the lateral atria *m*, which has already begun to split and form the muscles.

Fig. IX.—Embryo at about the time that the stolon appears.

Fig. X.—Stolon, at a very early stage, showing the ovaries *x*, *x*; [in this Figure the letters *a* and *b* were accidentally transposed, so that *b* represents the outer tunic, and *a* the branchial sac].



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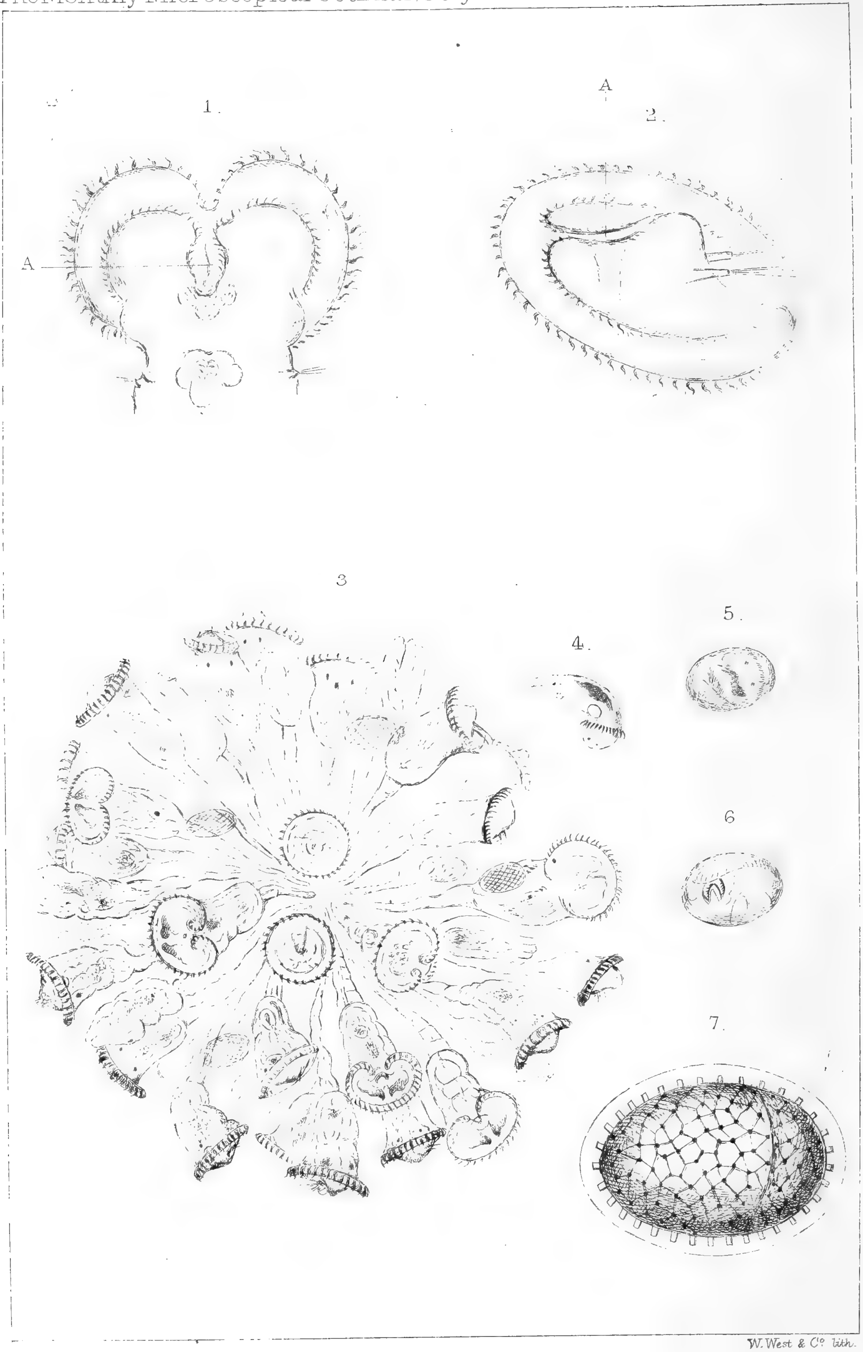
EMBRYOLOGY OF SALPA



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CONOCHILUS VOLVOX

V.—*On the Limits of the Optical Capacity of the Microscope.*

By Professor Helmholtz; with a Preface by Dr. H. Fripp.

The last number of the 'Proceedings of the Bristol Naturalists' Society' contained a translation of Professor Abbe's article on the "Theory of the Microscope," originally published in Schultze's 'Archives.' In that article Professor Abbe stated the general conclusions at which he had arrived after a prolonged investigation of the optical laws affecting the transmission of light through the lenses of the microscope. These laws relate to—1. The divergence of the rays of light forming a geometrical image; 2. The brightness of that image; 3. The dispersion of coloured rays, and its consequences; and 4. The diffraction of light occasioned by minute particles in the *objects* placed under (or before) the microscope. In explanation of these several phenomena, a theory of the microscope was stated in general terms, the mathematical demonstration of this theory, and its various applications, being reserved for a future communication.

Simultaneously with Professor Abbe's researches, a most interesting investigation of the same subject was completed by Professor Helmholtz, and appeared in Poggendorff's 'Annals' (1874). The theoretical grounds taken by these two authors are identical, and their results, so far as the researches were directed to the same points, also agree. But in each essay the mode of treatment is thoroughly independent, and the experimental proof of the conclusions respectively obtained is conducted by each writer in a separate and original method. The mathematical demonstrations omitted in Professor Abbe's article are fortunately supplied by Professor Helmholtz, and the two essays are confirmatory and supplementary to each other in several other respects, whilst in both we recognize that clearness of thought and precise knowledge of the subject treated, which justifies entire confidence in the conclusions. It seems therefore to me that Professor Helmholtz's essay should naturally follow in this number of our 'Proceedings.' For, taken together, these two essays form the most complete and authoritative exposition of the optical principles involved in the action of microscope objectives, and the most trustworthy interpretation of that action, and consequently of the capacity of performance of such objectives, that have as yet been made public.

In introducing the first of these essays to the notice of our readers, I expressed my strong conviction of its high value as a contribution of really scientific character to the theory of the microscope. The essay of Professor Helmholtz deals somewhat more fully with that aspect of optical science which is known as physiological optics, and of which no physicist of our times has a more

profound knowledge. This point of view had not been neglected by Dr. Abbe, but in my translation two short sections of his essay, which referred to brightness of image, and to certain inquiries connected with illumination of the image, were, for reasons mentioned in the preface, omitted. It is therefore so much the more satisfactory that Professor Helmholtz's essay enters fully into the subject. The peculiar conditions under which objects are seen when magnified by the microscope, can only be understood by studying both aspects, physical and physiological, in connection with each other. The laws of formation of optical images (when amplified by interposition of lenses), and the laws of dispersion of the rays by which these images are formed, help us to an interpretation of the physical agencies at work, and show us also why the extreme amplifications employed render vision through the microscope more imperfect than through any other optical instrument, such as telescope or camera. But the analysis of these physical agencies and effects involves the consideration of the eye itself, as an optical instrument through which the microscope image must pass to reach the perceiving organ. And apart from the imperfections arising from aberrations and dispersions of rays in the instrument, other imperfections of the retinal image will be found in considering the more or less favourable conditions under which the microscope image enters the eye. The area into which the microscope image is collected at the eye spot (over the ocular), varies in size with the amplification, and is smaller in proportion as the amplification is greater. And this variation of size is accompanied by variation in brightness of image and distinctness of detail. If the area of illuminated image entering the pupil is smaller than that of the pupillary aperture, loss of brightness is felt. For the condition of most effective illumination (brightness of image) is that which obtains when the area of image at the eye spot, and the area of the pupil, are equal. On the other hand, a small and intensely bright spot of light in front of the pupil presents the exact condition under which entoptic shadows obscuring the image are thrown with it on the retina. But as brightness of image is as necessary to distinct vision as any mere amplification of detail can be, it follows that a suitable relation of "aperture" to "magnifying power" must be maintained in every good objective; for "aperture" in this particular case means the measure of light admitted with the image-forming rays; and as a larger measure of light is required in proportion to the increase of magnifying power, so it is only when these two factors are suitably proportioned that details in the objective will be rendered clearly visible in its microscope image. And again, as respects the bundle of rays collected into a smaller or larger area at their entrance to the pupil, the regulation of illumination from without is better maintained with a large

"aperture" of objective by means of diaphragm openings and stops than by using stronger light with diminished aperture. Thus the management of illumination, and manipulation of the microscope to obtain good definition, though for the most part left to empirical practice, would be more easily and thoroughly acquired if the physiological laws were carefully studied. But another and far more serious deterioration of definition arises from excessive diminution of area of the image entering the pupil. This contracted area —the necessary consequence of the optical combinations used to obtain high amplification—has the same effect as any minute aperture through which a luminous object is viewed, and occasions, as is well known in physics, those diffractive effects which obscure the outlines of an image by making them overlap each other. On this fact is founded the whole argument of Professors Helmholtz and Abbe respecting the limits of microscopic vision, as well as the corollary which directly follows from it respecting the ultimate limits of minuteness to be assigned for vision of any and every kind of material atoms with the optical apparatus and materials yet employed. The theory of the microscope as interpreted by Helmholtz and Abbe on identical physical and physiological bases, is therefore of great importance in its general bearing on physical science, and the precise and comprehensive treatment of it in the following pages worthy of careful study.

As respects the translation now offered, it is only necessary to add that it was undertaken at the same time as that of Professor Abbe's essay, and with exactly the same motives. Our readers will, it is hoped, bear in mind that the translator's object was simply to make known to those who could not otherwise so readily inform themselves, the views of scientific men abroad, whose authority on these subjects is at all events high in their own country, and whose teaching he had himself accepted with pleasure. No mention of English cotemporary work was needed therefore in the brief introductory notice of Dr. Abbe's article. Since its publication, however, the translator has been questioned respecting English contributions to the theory of the microscope, and he therefore ventures to add a few words on this subject.

One may be well excused from referring to the meagre optical chapters in our handbooks on the microscope, which might perhaps suit the 'Boys' Own Book,' but which contain neither demonstration nor diagram of the course of rays through any sort of modern lens system, nor even a rough application of its very elementary statements respecting refraction and reflexion to any special formulæ of constructions, according to which the lens combination of an objective would be worked, or by which its performance would be tested. Nor can the favourite descriptive chapter of the instruments of various makers help anyone to a theory of the micro-

scope. The opinions expressed by experts and authorities on definition, penetration, resolution, aperture, &c., as being so many separate *powers* or qualities, besides savouring strongly of a mythological period in the history of the microscope, have only retarded the search in the right direction, viz. by physical analysis and physiological study of optical phenomena for true causes of the effects observed. And in fine it must be confessed that our handbooks fail greatly in respect to theories of the microscope, however valuable their information on practical and mechanical subjects, and more especially on all branches of science involving skilful *use* of the instrument.

In the absence of such handbooks as the German students possess, and of which the work of Nägeli and Schwendener might be cited with admiration as an example, the scattered articles and shorter notices in our serials rise into comparative importance. But it will scarcely be contended that such desultory and disconnected communications and such remarkable disputes respecting easily determined facts, should be accepted as an equivalent of the systematic theory and practical demonstration which distinguish foreign study of optics applied to the microscope, from our yet unlearnt, or at least unwritten, micrographic science.

Various communications bearing more or less on the optical capacity of lens systems constructed on given formulæ or for employment as "dry" or "immersion" objectives, have appeared in the 'Monthly Microscopical Journal,' the 'Quarterly Journal of Microscopical Science,' and the 'Transactions of the Royal Society' during present and preceding years. Of these, one series of papers published by Dr. R. Pigott claims to be a mathematical exposition of optical laws governing the divergence and dispersion of rays of light transmitted through different kinds of glass. Another series of papers by Mr. Wenham takes the practical direction to which English microscopists mostly incline. The communications of Mr. Sorby have enriched microscopic science with the most ingenious and successful applications of spectrum analysis that any country can boast. To all these gentlemen the English student may feel equally indebted for their respective labours. And the mention of these in juxtaposition with the work of so great an authority as Professor Helmholtz and so conscientious a workman as Professor Abbe, is not only due as a recognition of the individual services, but also as a proof of the higher direction of study now being pursued in England by amateur microscopists. As a humble member of this numerous class, the present writer ventures to refer to the early date of Mr. Wenham's communications when he stood almost alone as the pioneer of a future micrographic science, and to bear thankful testimony to the practical experience and sterling value of all

that he has written. And he also cordially recognizes the high aim and zealous study of Dr. R. Pigott, the direction of whose labours must ultimately prove most serviceable to all who desire to understand the real power and possible perfection of their favourite instrument. Any unfair spirit of criticism of matters so little appreciated by some of his critics is to be earnestly deprecated. One can only regret, whilst profiting by the opportunity of hearing all sides of a question, to be reminded of the woeful sentiment "*tantæne celestibus iræ.*" The vexatious partisanship of "aperture" and the disputed estimates of the performance of lenses constructed by this or that maker, must appear as overstrained and even ridiculous to the optician who can best gauge his own or any other maker's work, as to those who care only to understand the principles of construction and to form a rational judgment of their action.

It is to be hoped that a more general agreement on the essential parts of the theory of the microscope will soon prevail, and that the exaggerated significance of certain matters too long discussed in our journals, will fade to its proper vanishing point.

The Theoretical Limits of Optical Capacity of the Microscope.

In Poggendorff's 'Annalen' for 1874, Professor Helmholtz published an article, of which the following is a translation.

Whether, and to what extent, the optical performance of the microscope is capable of further improvement, is a question of the greatest moment for many branches of natural history. Doubtless, some progress, and notably through the revival of Amici's suggestion of immersion lenses adopted and carried out with such success by Hartnack, has been made, but each onward step is slow and faltering. We have, it is clear, arrived now at a point at which any trifling gain is effected with a disproportionate effort of mental as well as mechanical labour. And yet, so far as I can see, no one has been able to give any reason why this should be, excepting the common belief that the difficulty lies in overcoming the spherical aberration of lenses so small and of such quick curvation as is needed for objectives of very high magnifying power. It is not long since Herr Listing, one of the most eminent authorities on this subject, discussed * the means by which it might be possible to obtain amplifications ranging from 25 to 50,000 diameters, whilst in actual practice the ordinary range of *serviceable* amplification is at the present moment limited to from 400 to 800 diameters. Moreover, the collective experience obtained by repeated efforts of practical opticians has taught us that all high amplifications combined with good definition (i. e. sharp delinea-

* Poggendorff's 'Ann.' vol. cxxxvi.

tion) are obtainable only by instruments in which the objective admits a cone of light of very large angular aperture from each point of the object.

We have gradually arrived at that stage of improvement in the construction of instruments in which rays of light whose direction is nearly perpendicular to the axis of the instrument are passed into and through the objective, and transmitted towards the ocular. This, it is true, happens only when a lens is used dry (i. e. the front surface in contact with air), in which case rays inclined to the axis at angles up to $87\frac{1}{2}°$ actually do enter a well-constructed immersion lens. This angle, however, diminishes to about 48° * when the lens is used wet, that is, when water is dropped between lens and covering glass as in the ordinary practice. This last-named angle is nevertheless of far higher amount than any angle of aperture in the lens system of a telescope, or photograph camera, because with such oblique incidence the spherical aberration, even in the carefully calculated and accurately executed lenses of these instruments would be simply intolerable. Why then, notwithstanding this, is the large incident cone of light in the microscope more advantageous than a narrow one of more intense light which would deliver an equal absolute quantity? The answer hitherto given to this question appears to me unsatisfactory. For the so-called "penetration" (i. e. the power of delineating by light and shadow and so rendering visible to the eye particles whose refractive quality differs but slightly from that of the matter surrounding them) depends solely upon the proportion of the aperture of *illuminating* cone to that of the cone passing from points of the object into the lens. Sufficient delineating shadow can only be got by narrowing the aperture of the illuminating cone, and a comparatively large cone can only be applied beneath the object when the cones of light passing from it into the objective are also large.

Now there does, in point of fact, exist in the microscope a special cause which under the conditions here given produces a far greater aberration of rays from the focal plane than is occasioned by spherical and chromatic aberration, and which makes itself most felt just when the cones of incident light are smallest. This cause is diffraction.

If, perhaps, occasional allusion has been made to diffraction as a cause of deterioration of the microscopic image, I have yet nowhere found any methodical investigation into the nature and amount of its influence; but such an investigation shows, as will here appear, that diffraction necessarily and inevitably increases with the increase of magnifying power, and at length presents an

* These figures, it must be borne in mind, denote in each case the angle included between outermost incident ray and axis of instrument, that is, *half* the so-called "*angle of aperture.*"

impassable limit to the further extension of microscopic vision, which limit, moreover, has been already closely approached in our newest and best instruments.

That diffraction and consequent obscurity of microscopic image must necessarily increase with increasing amplifications of the image, and this quite independently of any particular construction of the instrument, rests as a fact upon a general law which applies to all optical apparatus, and which was first formularized by La Grange * for combinations of any kind of "infinitely thin" lenses. This law has apparently remained almost unknown, perhaps because La Grange enunciated it in equations whose coefficients have not characters which readily present clear ideas to the mind. In my treatise on physiological optics, I have given expression to this law in a somewhat more general form, namely, for centred systems of refracting curved surfaces with any singly refracting medium between them, and have endeavoured to formularize it in readily intelligible physical characters. I shall therefore recapitulate as briefly as possible this theorem and its demonstration. It holds good for every centred system of spherical refracting or reflecting surfaces through which rays pass with angles of incidence so fine as to form punctiform images of punctiform objects; that is to say, refracts homocentric rays, homocentrically.

By the term centred system, I designate one in which the centres of the curves of each refracting or reflecting spherical surface lie in the same straight line, the "axis" of the system. In front of such a system, and situate in its axis, let us suppose a luminous point belonging to some object lying in a plane at right angles to the axis, and from which rays pass through the system. The angle formed between any one of such rays and the axis, we shall call the divergence angle of that particular ray. Any plane supposed to extend through the axis and along the ray, constitutes the incidence plane of that ray at the first refraction, and will include, therefore, the same ray after its next refraction, and consequently after every subsequent refraction. Of this plane, which will be divided in crossing the axis into two halves, one half will be treated as positive, the other as negative, and in correspondence therewith, the divergence angle of the ray as positive or negative, according as the ray proceeds towards the positive or negative half of the plane. These postulates being settled, the rule may be thus stated:—

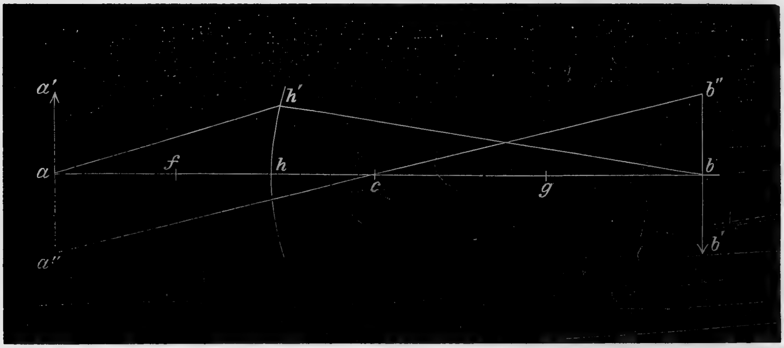
THEOREM.

In a centred system of spherical refracting or reflecting surfaces the product of the divergence angle of any ray, the refraction index of the medium through which that ray passes, and the

* "Sur une Loi générale d'Optique," 'Mémoires de l'Académie de Berlin,' 1803.

magnitude of the image to which the rays passing through that medium belong, remain unchanged by every refraction, provided always that the conditions of production of an accurate image are duly preserved. This product will therefore have the same value after emergence of the rays as it had before they entered the system of lenses.

DEMONSTRATION.



Let $a\,b$ be the axis of a lens system,
$h\,h'$ one of the refracting surfaces,
c the centre of its curve,
a the point of convergence of rays, incident on $h\,h'$,
b the point of reunion of rays refracted by $h\,h'$,
f the front principal focus,
g the back principal focus.

Further, let n' represent the ratio of refractions of the medium in front of $h\,h'$,
n'' represent the ratio of refractions of the medium behind $h\,h'$,
a' the positive divergence angle $h'\,a\,h$ of the ray passing in first medium through h',
a'' the negative divergence angle, in second medium $-h'\,b\,h$,
β' the magnitude of image $a\,a''$ belonging to the rays of the first medium,
β'' the magnitude of image—$b'\,b''$ belonging to the rays of the second medium.

Firstly, we have, from similarity of triangles $a\,a''\,c$ and $b\,b''\,c$,

$$\frac{\beta'}{\beta''} = -\frac{a\,c}{c\,b}. \qquad [1]$$

Again, if we consider the short arc $h\,h'$ of the refracting surface as a straight line at right angles to the axis $a\,b$,

$$h\,h' = a\,h\,.\,\tan.\,\alpha' = -\,b\,h\,.\,\tan.\,\alpha''.$$

Or substituting the angles for the tangents, which is allowable here on account of the smallness of the angle,

$$\frac{\alpha'}{\alpha''} = \frac{b\,h}{a\,h}. \qquad [2]$$

Multiplying equations [1] and [2], we get

$$\frac{\alpha'\,.\,\beta'}{\alpha''\,.\,\beta''} = \frac{a\,c\,.\,b\,h}{b\,c\,.\,a\,h}. \qquad [3]$$

Now, according to the known laws of refraction at a spherical surface, whose radius $h\,c = r$, the value of their principal focus is

$$\mathrm{F}' = h\,f = \frac{n'\,r}{n'' - n'}; \quad \mathrm{F}'' = h\,g = \frac{n''\,r}{n'' - n'}; \qquad [4]$$

from which follow

$$\frac{\mathrm{F}'}{\mathrm{F}''} = \frac{n'}{n''}; \qquad [4^{a}]$$

$$\mathrm{F}'' - \mathrm{F}' = r. \qquad [4^{b}]$$

Further,

$$\frac{\mathrm{F}'}{a\,h} + \frac{\mathrm{F}''}{b\,h} = 1, \quad \text{and} \quad \frac{\mathrm{F}''}{a\,c} + \frac{\mathrm{F}'}{b\,c} = 1;$$

or

$$\frac{b\,h}{a\,h} = \frac{b\,h - \mathrm{F}''}{\mathrm{F}'}, \quad \text{and} \quad \frac{b\,c}{a\,c} = \frac{b\,c - \mathrm{F}'}{\mathrm{F}''}.$$

Division of the last two equations gives

$$\frac{b\,h\,.\,a\,c}{a\,h\,.\,b\,c} = \frac{\mathrm{F}''\,(b\,h - \mathrm{F}'')}{\mathrm{F}'\,(b\,c - \mathrm{F}')};$$

but by equation [4ᵇ],

$$b\,h = b\,c + r = b\,c + \mathrm{F}'' - \mathrm{F}',$$

and

$$b\,h - \mathrm{F}'' = b\,c - \mathrm{F}'.$$

Hence

$$\frac{b\,h\,.\,a\,c}{a\,h\,.\,b\,c} = \frac{\mathrm{F}''}{\mathrm{F}'} = \frac{n''}{n'}, \text{ according to equation } [4^{a}].$$

Therefore equation [3],

$$\frac{\alpha'\,.\,\beta'}{\alpha''\,.\,\beta''} = \frac{n''}{n'},$$

or

$$n'\,.\,\alpha'\,.\,\beta' = n''\,.\,\alpha''\,.\,\beta''. \qquad [5]$$

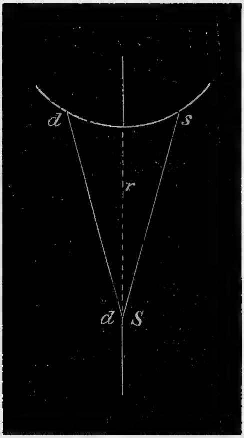
q. e. d.

From this theorem it follows—

Firstly, that when a ray, B, proceeding from a luminous point has an absolute smaller divergence angle than the ray A, the divergence angle of B will, after subsequent refraction, remain always less than that of A, because the product obtained by our theorem for B is from the beginning less than that obtained for A, and for the same reason must continue to be smaller after each refraction.

Secondly, when two rays, starting from the same point on the axis, with equal angles of divergence, but following planes which extend in opposite directions through the axis, their divergence angles continue to be equal after each refraction, a result which appears indeed at once evident from the symmetrical disposition of a lens system round its axis.

If now we imagine the illuminating rays, on their way to the object, to be circumscribed by interposing a diaphragm pierced with a circular opening whose centre coincides with the axial line, the plane of the diaphragm being at right angles with the optical axis, then those rays which pass through the opening close to its margin have all alike the largest divergence angle, and retain the same relation after each fresh refraction. These rays obviously occupy the exterior outline of cones having a circular base, and whose axis is the optical axis of the lens system, and they constitute the boundary of the cone of light proceeding from the luminous point. The divergence angle of these border rays is, in this case, throughout their entire course, the angle which the semi-aperture of the conical surface bounding the illuminating cone measures.



From this there follow, firstly, certain important results in regard to the *photometric* conditions of the microscope image.

According to known laws of photometry, we may equate L the quantity of light sent forth from the luminous point dS upon another point ds, whose distance is r, as follows, where (r, N) and (r, n) represent the angles formed between the line r and the normals N and n.

$$L = J \frac{dS \, . \, ds}{r^2} \, . \cos.(r, N) \, . \cos.(r, n). \quad [6]$$

If now we understand by ds the circular aperture of the cone of rays at one of the refracting surfaces, and by dS a luminous point intersected by the axis so that r falls in the axial line,

Then cos. $(r, n) = 1$, and $d\,S\,.\,\cos.\,(r, N)$ is the projection of $d\,S$ on a plane normal to the axis.

Let a be the angle of divergence of the rays directed to the periphery of $d\,s$, then $d\,s = \pi\,.\,r^2\,.\,a^2$.

$$L = J\,.\,\pi\,.\,a^2\,.\,d\,S\,.\,\cos.\,(r, N). \qquad [6^a]$$

The same amount of light must also be contained in the same cone of rays continued through the following medium. And if we indicate the corresponding quantities by the signs J', a', $d\,S'$, N', then

$$L = J'\,.\,\pi\,a'^2\,.\,d\,S'\,.\,\cos.\,(r, N'). \qquad [6^b]$$

Now, $d\,S'$ is the image of $d\,s$, and its projection—normal to the axis—$d\,S'\,.\,\cos.\,(r, N')$ is the image of the corresponding projection of $d\,S$. We have therefore the proportion

$$d\,S\,.\,\cos.\,(r, N) : d\,S'\,.\,\cos.\,(r, N') = \beta^2 : \beta'^2.$$

From which follows

$$J\,.\,a^2\,.\,\beta^2 = J'\,.\,a'^2\,.\,\beta'^2;$$

and by equation [5],

$$J : J' = n^2 : n'^2. \qquad [6^2]$$

This gives the brightness with which the surface of image included within the outline of the illuminating cone shines, independent of the direction which $d\,S$ and $d\,S'$ have in relation to the axis, and of their distances from the surface of the curve (of lens).

From this image ($d\,S'$) we might pass on to consider a second, $d\,S''$, and so forth. It is obvious that between each following image and $d\,S$ a similar equation would arise.

If we suppose the object and the image to lie in the same medium, then *the brightness of the optical image produced by rays which incline at very slight angles to the axis and perpendicular will always be equal to* (i. e. *neither more nor less than*) *the brightness of the object, except in so far as loss of light by reflexion and absorption may occur.*

But this law should hold good without limitation of divergence angle. For if it were possible to throw an image of any bright point sending forth its light according to the conditions above expressed (namely, of rays circumscribed by a diaphragm aperture), which image should shine with greater intensity than the rule above given admits; then we could cause this bundle of rays to pass on as parallel rays through a plane end-surface into the air, and to fall into the eye of an observer; and in such case it would happen that an object would be seen more brightly illuminated through an

optical instrument than it was before,—a thing contrary to all experience, whatever kind of transparent refracting material be used. Now, if this were possible with light it would also be true of heat, as might be shown by application of similar reasoning; and then the law of equal radiation of bodies possessing equal temperature would be impugned.

But the equation which premised very slight divergence angles of incident rays may be more precisely formulated, and so express the same result in the case of wide divergence angles.

A more precise expression of the Law of Divergence Angles.—In equation [5] it is a matter of indifference whether we substitute for α its sine or tangent or similar functions which for indefinitely small α would be its equivalent. If we assume larger divergence angles of a pencil of rays whose section is a circle, then

$$L = J\, d S \int_0^\alpha 2\,\pi \,.\, \cos.\, \alpha \,.\, \sin.\, \alpha \,.\, d\,\alpha = \pi\, J\, d\, S \,.\, \sin.^2\, \alpha.$$

If after a series of refractions the surface $d\,S_1$ is completely and accurately imaged in $d\,S_1$ with the brightness $\frac{n_1^2}{n^2} J_1$ and α_1 of the respectively appertaining divergence angles, then the amount of light must be

$$L = \pi\, J\, \frac{n_1^2}{n^2} \,.\, d\, S_1 \,.\, \sin.^2\, \alpha_1 \,.$$

As now, $d\,S : d\,S_1 = \beta^2 : \beta_1^2$, there follows from these equations,

$$n \,.\, \beta \,.\, \sin.\, \alpha = n_1 \,.\, \beta_1 \,.\, \sin.\, \alpha_1 \,, \qquad [7]$$

which renders this formula of equation [5] valid for larger angles of divergence, assuming that β and β_1 are two images exactly reproducing each other, and whose surfaces are perpendicular to the axis.

Brightness of Image.—When the pupil of the observer's eye is fully immersed in the pencil of rays proceeding from any point of an image, the observer will see the image illuminated as brightly as the object. This result was already announced by La Grange. Unfortunately he had not investigated a second case, which happens to be more common just when high powers are used, namely, when the pencil of rays does not entirely occupy the pupil of the eye.

If a pencil of light having only small divergence angle α_1 does not entirely fill the pupil when the image β_1 is situate at the proper distance of distinct vision, then the brightness H of the retinal image in that eye will be less than that entering the free eye H_0, whose pupil is entirely filled with light.

Let s indicate the distance of vision, p the radius of the pupil, then the area of its surface will be πp^2, the cross section of the pencil of light $\pi s^2 . \sin.^2 \alpha_1$ and the general relation will be

$$H : H_0 = s^2 . \sin.^2 \alpha_1 : p^2.$$

Or using equation [7],

$$H = H_0 . \frac{s^2}{p^2} \cdot \frac{n^2}{n_1{}^2} \cdot \frac{\beta^2}{\beta_1{}^2} \sin.^2 \alpha.$$

The last medium in front of the eye must necessarily be air, therefore $n_1 = 1$, and if we indicate by α_0 the angle of divergence of the instrument measured in air according to Lister's method, then $\sin. \alpha_0 = n . \sin. \alpha$. Putting the amplification $\frac{\beta_1}{\beta} = N$, then

$$H = H_0 \frac{s^2 . \sin.^2 \alpha_0}{p^2 . N_2}.$$

With an amplification N_0 by which the cone of light just fills the pupillary opening, and which we shall call the normal amplification of the instrument, $H = H_0$. Hence

$$N_0 = \frac{s}{p} \sin. \alpha_0 . \qquad [8]$$

And if α_0 remains constant,

$$H : H_0 = N_0{}^2 : N^2. \qquad [8^a]$$

If as was assumed

$$N > N_0.$$

Whilst $H = H_0$ when

$$N \leqq N_0.$$

That is to say,

*The brightness of an image seen through the microscope is equal to that of light filling the unoccupied eye * when the amplification is less (or not greater) than the "normal" amplification* (i. e. when the area of the ocular image just fills the pupil); *otherwise, with the same constant divergence of incident rays, the brightness is inversely proportional to the amplification of image.*

The normal amplification increases with the increase of the sine of the divergence angle whose greatest value is 1 when this angle approaches a right angle (as is the case with the widest-angled objectives).

Assuming 10 inches as the distance of clear vision for calculation

* Daylight is of course supposed, and a monocular microscope in use.

of the amplified image, and $1\frac{1}{2}$ mm. as radius of pupil for bright illumination, the normal amplification is represented by the figures 166·7, and the brightness of image follows the following rates:

For an amplification of	333·3	$\frac{1}{4}$ brightness.
" "	500·0	$\frac{1}{9}$ "
" "	666·7	$\frac{1}{16}$ "

which shows how rapidly the brightness must necessarily decrease with increasing amplifications.

Were it possible to conduct a hemispherical cone of light from an object lying in water into an immersion lens, and form therewith a correct image, all these amplifications might be raised in the proportion 1·335 to 1 whilst the brightness of image remained the same. But, as already remarked, every instrument hitherto constructed admits in air only, and not in water, a cone of incident light at all approaching to the hemispherical (180°).

The sectional area of the pencil of light entering the pupil may be determined empirically with ease. Focus the instrument on a bright field, and withdraw the eye from the ocular (keeping the direction of the axis of the microscope) and look at the ocular itself. Just in front of it will be seen a small bright circle against a dark ground. This is the optical image of the objective lens which the ocular (i. e. chiefly its field glass) forms. All light which comes through the objective and has passed the ocular must be collected in this image of the objective. It corresponds, therefore, to the area in which the several cones of light, transmitted from the bright points of the object, are collected at this spot. To gather all this light and thus get the largest and clearest field of vision, the pupil of the eye must be brought to this spot. The relation between the area of the image and that of the pupil gives at once the ratio by which the brightness of the image is less than that of the object when looked at with the unarmed eye. The same brightness of image as of object exists only when the size of the image is equal to or larger than that of the pupil.

In the instance of the telescope, La Grange had already stated that the relation of size between the diameter of the objective and that of the picture of the objective formed by the ocular, is directly as the amplification, and he proposed to employ this ratio as a means of determining the amplification. With the telescope, however, such a decrease of brightness is not a necessary accompaniment of increased amplification, because the amount of incident light may be augmented indefinitely by enlarging the object-glass or reflector. The aperture of the cone of light entering the microscope is, on the contrary, definitely restricted by the limits of the angle measuring that aperture.

So far, our demonstration shows that the relation between brightness of image and amplification is entirely independent of any particular construction of the instrument, provided only that it gives well-defined images. An increase of amplification would only be possible, therefore, when a more intense illumination, e. g. direct sunlight were employed, as indeed Listing had in view in the methods proposed by him for obtaining enormous amplifications. But here other difficulties present themselves, which arise from the very slight divergence angle of the emerging rays, as appears in all cases of high amplification from the conditions of the equation representing the course of rays that enter an objective with wide divergence angle.

The first difficulty is, that shadows of entoptic objects throng the field more densely as the area of this field at the eye spot (ocular image of the objective) becomes smaller. The retina is illuminated from this area as if it were the source of light from which proceeded all the rays that enter the eye. This area is at the same time the basis of the collective pencils which belong to the several points of the object, and of its image on the retina, and its diameter, as before shown, varies in inverse proportion of the amplification. But the very conditions which must be fulfilled in order to obtain sharply defined shadows of objects within the eye are exactly what occur here, namely, that a strong light should enter the eye from a relatively small surface.

Whoever has, at any time, attempted to illumine the field of the microscope with direct sunlight, when employing a high amplification, will remember the peculiar spotty appearance of the field so obtained. Some of these spots remain fixed in the field, but others move with the motion of the eye. The first class of spots is due to dirt particles or imperfect polish of the ocular lenses; the second arises from shades caused by intervening opacities in the tissues of the eye—conjunctiva, cornea, crystalline lens, or vitreous humour.* This method has even been used to discover their existence, and is, in truth, a very suitable one. In proportion, however, as entoptic objects become more noticeable, will a greater number of finer details of microscope objects become obscured.

A second and inevitable disadvantage arising from the narrow divergence angle of the emerging rays shows itself in the occurrence of *diffraction phenomena*, whereby the outlines of visible objects are effaced, and at the same time doubled or further multiplied. We have to deal here chiefly with diffraction phenomena as they appear when we look through a minute circular opening. A bright point of light (reflexion of sun on the bulb of a thermometer) viewed through a pin-point hole pierced in a card appears as a

* But mainly from the retinal vessels, as shown by Heinrich Muller, vide Wurzburg Verhandlungen,' vol. v. p. 411.—H. E. F.

bright disk surrounded by alternate bright and dark circles. The apparent breadth of these rings, reckoned from minimum to minimum, corresponds very nearly to a visual angle whose sine is equal to $\frac{\lambda}{a}$, where λ expresses the respective wave-length of the light, and a the diameter of the opening. The outermost rings have exactly these dimensions, the inner are a little wider, and the radius of the innermost bright ring is $1 \cdot 220 \frac{\lambda}{a}$. Now, as the smallest visual angle under which we can possibly distinguish two fine bright lines from each other may be fixed at 1 minute, the figures of the brightest yellow-green light, whose wave-length $= 0 \cdot 00055$ mm.; will be visible when $d = 1 \cdot 89$ mm. Even with a somewhat larger opening the dispersion of a bright point into a circle or of a bright line into a streak must be noticeable.

When we look through such an aperture at any object which shows luminous points, the diffraction figures of the separate points partially cover each other, so that the fringe of dispersion circle of each single point, taken by itself, may not be recognizable. The effect, however, of this diffraction, since it changes every point into a small dispersion circle, obviously causes effacement of the true outline, just as happens when the accommodation of the eye is imperfect, in consequence of which very minute objects, which can be perceived only when the image on the retina is sharply defined, are unrecognizable. We may convince ourselves that this is the fact by a simple experiment. The retina is most sensitively impressed by such objects as gratings, consisting of alternate dark and light parallel lines, whether printed on paper, or made of wire-work, or drawn on glass. Let the observer place himself at such a distance from the grating that, with the aid of spectacles giving perfect accommodation of the eye, he may just be able to distinguish the bars or lines separately from each other. Then let him place before his eye a card in which fine apertures of different diameters have been pierced, and observe whether he still sees the lines or sees them as well with as without the card. The grating must be brilliantly illuminated (e. g. by exposing lines printed on paper to direct sunlight), in order that the picture seen through the aperture may remain sufficiently bright. On trying the experiment myself, I find that a notable deterioration of the image is caused by an aperture of $1 \cdot 72$ mm. diameter, and the deterioration is much more striking with still narrower apertures.

Instead of a series of lines printed letters may be used, the same conditions being fulfilled, namely, by observing the point at such a distance that the single letters may be just distinguished. On looking at them through an aperture of 1 mm. diameter, they will be scarcely or not at all legible. This experiment is, however, not

so sensitive as the first. But in all cases the best accommodation of the eye must be carefully maintained, otherwise the act of passing a card, pierced with an aperture, before the eye may, when there is imperfect accommodation, actually improve vision by diminishing the dispersion.

The theory of diffraction of rays in the microscope leads, as will be shown in the following pages, to the conclusion that any single point of light in *the object* must, when viewed through the microscope, appear exactly as if an actual luminous point, situate in the *image of the object*, were observed through an aperture corresponding in size and position to the ocular images (at the so-called eye spot) of the respective narrowest diaphragm aperture.

Hence it follows, firstly, that diffraction phenomena must be visible when the ocular image has a diameter less than 1·89 mm., and that the size of the dispersion circle, caused by diffraction, must increase in inverse proportion to the diameter of this ocular aperture, consequently in direct proportion to the amplification, supposing that the incident light from each point in the object remains unchanged. Under such circumstances then, the image will not, even with higher amplifications, suffer *further* loss of sharpness of outline from diffraction, inasmuch as the dispersion circles preserve, throughout, the same relation to the apparent magnitude of the object. On the other hand, the deterioration arising from diminished brightness and multiplication of darker entoptic shadows, must increase with the amplification. From this it follows, therefore, that, as a general rule, that amount of amplification will show most detail by which the minutest points that are visible at all in the image, shall be presented under the most suitable visual angle, namely, somewhat larger than that at which an observer can distinguish the minutest objects visible to him under any circumstances.

Calculated by the equation before mentioned, the diameter (1·89 mm.) of the area of light rays entering the pupil, when the light incident on the objective (in air) spreads out to nearly 180°, corresponds to an amplification of $264\frac{1}{2}$. For objectives with less aperture the amplification must be set down at a lower figure. In H. v. Mohl's handbook of the microscope it is stated that amplifications varying between 300 and 400 allow most detail to be seen, whilst Harting, speaking of more recent instruments with large angular aperture, found amplifications of 430 to 450 most serviceable.

If now it be required to determine the magnitude of the minutest recognizable object as a standard by which to measure the accuracy of the microscopic image, we must not take for our unit the measured diameter of such objects as bright single spots or lines on

a dark field, or *vice versâ*, for the reasons which I have already given in my 'Handbook of Physiological Optics' (p. 217), in discussing the capacity of the eye for distinct vision. For in the cases above mentioned the result depends not only on the proportional magnitudes of the images, but also on the susceptibility of the retina to slight differences of light. The most suitable objects are, here also, fine gratings which show alternate clear and dark stripes. Such indeed are in common use, as in the examples of Nobert's lines, and the line systems of diatoms and insect scales. But as the light of the bright stripes is doubtless strongly dispersed before it becomes quite undiscernible, dependence can be placed only on the measurement of the space between the centres of two contiguous stripes, and not upon the measurement of space occupied by the stripes (wide or narrow) as originally distributed. I select, therefore, as the measure of the minutest distinguishable objects, that smallest appreciable interspace between the centres of two contiguous stripes by which these stripes can still be recognized as separate.

When diffraction is caused by a fine network of square meshes, it can be proved that the network must appear as a uniformly illuminated surface when the breadth of fringe of diffracted light is equal to that of the open space of the network. For circular meshes, the integration for calculating the distribution of light is tediously diffuse. When the diameter of a circular mesh is equal to the length of one side of a square mesh, the outermost fringes in the spectrum of a bright spot are of equal width, but the innermost fringes are wider in the circular meshwork. If therefore the fringes of the square meshes are so broad as to efface all impression of separate bright lines of the network when the measured widths of fringe and mesh are equal, the same thing must happen with the circular meshwork, a portion of whose diffraction fringes is still wider. For this reason I have, in the following demonstrations, taken the width of the outermost fringes of a circular meshwork as the lower limit of distinguishable distances in an object. It is not, however, impossible that by some fortuitous overlapping of images, objects of still smaller dimensions might occasionally be half seen, half guessed at. But safe and certain recognition will scarcely be possible.

Let now

ϵ be the magnitude of the smallest recognizable interspace,

λ wave-length of the medium,

a divergence angle of the rays incident in that medium,

λ_0 a_0 the values of the last-named magnitudes (λ and a) for air.

Then by the formulæ deduced in a subsequent page,

$$\epsilon = \frac{\lambda}{2 \sin. a} = \frac{\lambda_0}{2 \sin. a_0}.$$

For white light we may, as before, take the wave-length of the medium bright rays.

$$\lambda_0 = 0 \cdot 00055 \text{ mm.}$$

If $a_0 = 90°$, then

$$\epsilon = \frac{\lambda_0}{2} = 0 \cdot 000275 \text{ mm.} = \tfrac{1}{3636} \text{ mm., or } \tfrac{1}{92000} \text{ inch.}$$

Were it possible to obtain with an immersion lens the transmission of rays = 180° of divergence aperture (in water), a would then = 90° and λ nearly $\frac{3}{4}\lambda_0$; and hence

$$\epsilon = \tfrac{1}{4848} \text{ mm.} = (\tfrac{1}{122000} \text{ inch).}$$

According to measurements of Harting,* the magnitude of the smallest distances taken with No. 10 objective of Hartnack, reckoned by our formula, is

$$\epsilon = \tfrac{1}{3313} \text{ mm.}$$

The figures $\frac{1}{5210}$ mm. given by Harting refer to the width of the dark space *between* the lines. In close accordance with the above are the measurements by Herr L. Dippel,† of fine diatoms, who found that the closest series of lines that he could distinguish $= \frac{1}{3500}$ mm., and the finer Nobert lines $= \frac{1}{3600}$ ($\frac{1}{90000}$ inch). Earlier measurements, 1853, of Messrs. Sollitt and Harrison, ‡ count much higher. Recognizable lines of *Navicula Arcus* are said to have been counted at 5120 to the mm. ($\frac{1}{129000}$ inch). This far exceeds the theoretical limits for objects in air. But since all later measurements remain much lower than these, I do not know that they are trustworthy. Harting also, who cites them, doubts their accuracy.

Besides any possible further increase of angular aperture in the case of objects lying in water, the capacity of performance might perhaps be increased by employing blue rays only. §

In photography, blue light is chiefly active, and photographs appear actually to perform more than the eye can with white light. In a photograph of *Surirella gemma*, executed by Dr. Stindi, with an objective of Gundlach's, giving $\frac{1000}{1}$ amplification, lines are visible which may be counted at 3800 to 4000 in the millimeter ($\frac{1}{100000}$ of English inch).

Thus it appears to me beyond doubt that diffraction of the rays is the principal cause of the limitation of sharpness of the microscope image. In comparison with diffraction, chromatic and spherical

* Published in vol. cxiv. of Poggendorff's 'Annals.'
† In his work on the Microscope: Brunswick, 1867.
‡ 'Quarterly Journal Microscopical Society,' vol. v. p. 62.
§ Hartnack made an illuminating apparatus for use of blue rays only, and exhibited it in the Vienna Exhibition, 1874.

aberrations appear to exert but an inconsiderable influence, in spite of the very large angles of incidence and divergence of rays. Considering the extreme care expended on calculation and execution of lenses for telescopes and the photograph camera, it is justly a matter of surprise that with the lenses of the microscope, which are so much more difficult to construct according to prescribed dimensions, and which have so large an aperture, spherical aberration makes itself so little felt. I have, however, already pointed out that when there is water between the object and covering glass, and also between this and the objective, the divergence angle is not $87\frac{1}{2}°$, as usually stated, but only $48\frac{1}{2}°$. With dry mounted objects an angle of $87\frac{1}{2}°$ can indeed be in action, but *only through the minute distance between the object and covering glass,* so that the spherical aberration arising therefrom is of no importance.

As wide pencils of light are needed to keep diffraction within moderate limits, the illuminating apparatus should also be capable of emitting pencils of the same angle, in order to show clearly the contour lines of dark objects.

If there happen to be particles in the object which act like lenses, these may of course convert a small illuminating pencil of rays into strongly divergent rays, and so become clearly visible. Otherwise nothing is seen but a confusion of diffractions at and in the object on one part, and in the (optical) aperture of the microscope on the other part.

Here lies obviously the explanation why microscopes, otherwise good, but whose illuminating apparatus is not specially arranged for the purpose, yield, with artificial illumination, e. g. a flame, such unserviceable images of the outlines of dark objects. For an immersions lens, the best illuminating apparatus is one constructed according to the same principle—that is to say, a lens of the same kind reversed. The readiest mode of finding whether the illuminating apparatus gives sufficiently wide pencils of light is to examine the ocular image with a magnifying lens after the instrument has been focussed.

I must now relate here the *failure of an attempted improvement,* the negative result of which is significant. I thought myself justified in inferring theoretically that the diffraction of the microscope might be neutralized if the points of the narrow aperture which causes this diffraction were made singly and separately luminous, and that this could be effected by causing a sharply defined optical image of the source of light (e. g. sun illumined cloud) to be thrown by a lens on the plane of this aperture. Years ago I tried experiments of this kind on a Nobert microscope, provided with immersion lenses, giving excellent definition. The result of this trial showed that it was perfectly indifferent whether the image of the source of light fell on the plane of the object or

of the objective. The diffraction fringes caused by the use of a very deep ocular remained uncorrected. More recently I have convinced myself by fresh trials made with larger lenses, that such a procedure is useless. When a good achromatic lens of about 18 inches focus is so placed as to show a sharp image of the source of light (as in this case a bright sky cloud) upon the surface of a system of lines scratched on glass, the images of many separate luminous points will be thrown upon the variously transparent clefts of this grating, and it might be supposed that the interference of rays which had passed through neighbouring clefts would cease. If, however, we look through the grating towards the lens, and place before the lens pieces of card pierced with fine slits, we see with the naked eye just the same diffraction fringes, as well at these slits as at the outer edges of the cards, as would be seen if the lens were removed, or the grating set out of focus.

Instead of the lines, I then made trial of two fine linear slits cut in cardboard, with an interspace of about 1 mm., and through which I could see with the naked eye a system of very fine interference lines belonging to the diffraction image of another slit which was cut with the lines at a very small acute angle, sufficiently narrow to produce the interference lines at the point of this angle. But these did not disappear when I threw an optical image of the incident light on the plane of the double (parallel) slit. In this experiment not the slightest suspicion could be entertained that chromatic or spherical aberration had dispersed the rays over an interspace of 1 mm. width. The only explanation I can offer is, that the light from the lens which passed through the acute angle of the slit serving here as object, suffers so strong a diffraction that it subsequently reaches the two openings of the doubly-slit card with a corresponding wave-phase and therefore sends interfering bundles through both openings. In order to be able to see the interference lines, it is necessary that their minima shall appear at a wider distance from each other than the width of the lines of which they are images, and when this condition is fulfilled theory does in fact show that the central clear portion of the diffraction figure of the simple slit forms a line of light which is broader than the distance between the two slits of the doubly-slit card.

Similar relations take place (although more difficult to subject to calculation) when the fine edge of a dark screen is used as the object. It is known that from such an edge, bundles of interrupted rays (in linear formation) likewise bend themselves into the dark field, which have corresponding phases of movement, and so when bent by a second screen can exhibit regular interference. That the resultant effect cannot become *nil* appears clearly from the fact that the effect of a bright line may be represented as the product of the action of two endless half-planes bounded by straight

lines the edges of which half-planes slightly overlap each other, minus the action of an equally bright whole plane. As the latter causes no interference phenomena, the bright line of itself could not cause interference in any part of the field, unless each of the half-planes also produced such interference. It follows therefore that the light bent away from a straight edge must also spread itself out with notable strength to the same width as would the light from a slit in the card bounded by two other slits.

Theory of Diffraction in the Microscope.

In conclusion, I shall here show a method by which the diffraction of rays passing through the microscope may be theoretically calculated. Instead of the simple lengths of rectilinear rays, as taken into consideration by the theory of diffraction of light which passes through one medium only, the *optical lengths* of the rays must be taken, that is to say, the lengths obtained by adding together the product of each portion of a ray multiplied by the index of refraction of the medium through which it passes.

The wave-phases of two rays that have started from the same luminous point, and have equal optical lengths, are also equal at the other terminal point, because the wave-lengths in different media are inversely proportional to the refractive indices. Further, it is known * that the optical length of all rays between two conjugate foci of the same pencil in which a perfect reunion of these rays is accomplished is equally great.

In order to calculate the diffraction through the (relatively) narrowest aperture of the microscope, each point (c) in the plane of this aperture must be treated as a ray centre whose phase is determined by the optical length of the normally refracted ray, which, starting from the luminous point (a), has arrived at c. This length I designate with $a\,c$. On the other hand, the difference of phase between c and the point b in the surface of the image whose brightness is to be determined depends on the optical length $c\,b$ found for the normally refracted ray travelling from c to b. The phase of movement continued from a, through c as a new centre of the ray, to b, will therefore depend on the sum of the optical lengths $a\,c + c\,b$. The share which this ray has in the movement in the point b will be given by an expression in the form

$$\text{A sin.}\left\{\frac{2\,\pi}{\lambda}\left[a\,c + a\,b - a\,t\right] + \text{constant}\right\},$$

where λ is the wave-length in empty space, A the speed of progressing movement, t the time. The sum of these quantities

* The proof of the law here adduced is to be found in my 'Handbook of Physiological Optics,' and elsewhere.

taken for every point c of the aperture (in which the factor a can be considered as approximatively independent of c) will finally determine the movement at b.

If now we suppose the rays passing from (a) and (b) to the point (c) of the relatively narrowest aperture to be prolonged in the direction which they have at the point (c) until they intersect each other in the points (α) and (β), these last points will be the images of the points (a) and (b), formed in the medium of (c). Since, then, from what has been said above, the optical lengths ($a\,\alpha$) and ($b\,\beta$), being lengths measured between conjugate foci, are constant, we may put

$$(a\,c) = (a\,\alpha) - (c\,\alpha)$$
$$(c\,b) = (\beta\,b) - (\beta\,c).$$

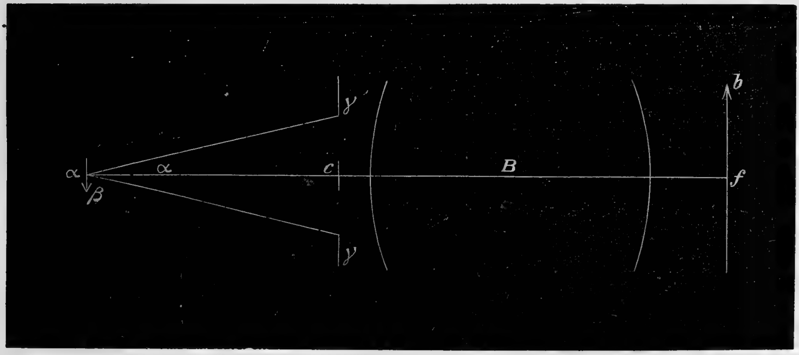
The direction of movement of the ray must be conceived as always advancing from the first to the second letters; and therefore

$$(c\,\alpha) \text{ be put } = -\,(\alpha\,c), \text{ as also } (\beta\,c) = -\,(c\,\beta).$$

Then the expression for the effect of each separate ray on the point (b) becomes

$$\text{A sin.}\left\{\frac{2\,\pi}{\lambda}\left[(\alpha\,c) - (\beta\,c) - \frac{t}{a} + (a\,\alpha) + (\beta\,b)\right] + \text{constant}\right\}.$$

The only terms amongst the signs bracketed under the sine that vary with the point c are $(\alpha\,c) - (\beta\,c)$. These optical lengths, however, lie wholly in the medium of (c), and are therefore straight lines; consequently, the diffraction effect of the light from



(a) at the point (b), apart from the factor A, which expresses its total intensity, will be the same as that of the light from α for the point β. But the latter can be calculated according to the known method valid for rectilinear rays.

Let $\gamma\gamma'$ be the relatively narrowest aperture, and (c) its middle point, B the portion of the optical system immediately behind this aperture, and let a be the image of the axis point a of the object; further, let $a\,\beta$ be its image lying in the medium $\gamma\,\gamma'$ and $f\,b$ the image formed by B in the last medium.

When light proceeds from a, and is viewed through the aperture $\gamma\gamma'$ whose radius is ρ, interference fringes will appear around a, in which the distance δ between each two neighbouring maxima (excepting the two first) will be according to known laws, if, as before, a represents the divergence angle $c\,a\,\gamma$, which by assumption is very small.

$$\delta = \frac{(a\,c)\,\lambda}{2\,\rho} = \tfrac{1}{2}\,\frac{\lambda}{a}.$$

If N be the amplification of the image $b\,f$ in comparison to $a\,\beta$, the breadth of fringe δ' of $b\,f$ will be

$$\delta' = \mathrm{N}\,\delta = \tfrac{1}{2}\,\mathrm{N}\,\frac{\lambda}{a} \qquad [8]$$

or as $\mathrm{N} = \dfrac{n\;a}{n'\;a'}$ when a' expresses the divergence angle of the emergent ray, n' the refractive index of the last medium, n that of the medium at (c).

$$\delta' = \frac{n}{2\,n'} \times \frac{\lambda}{a'}. \qquad [8^{a}]$$

If $n = n'$, then the form in which this value of the breadth of fringe of image $b\,f$ is expressed is exactly analogous with that for $a\,\beta$, and shows that the fringes in the last image are of just the same dimensions as if seen through the aperture which determines the divergence angle a' of the cone of rays $\gamma\,a\,\gamma$, or, in other words, through the ocular image of narrowest aperture.

The above demonstration presupposes that the relatively narrowest aperture of diaphragm is situate where the divergence angles of the pencil of rays are very small. It may, however, be situate at any part of the instrument. With an immersion microscope this condition is indeed not fulfilled when the surface of front lens is the relatively narrowest aperture. But it would be fulfilled if the aperture were situate on the upper side of the second or third lens. Thus if there were no lateral outspread of the advancing rays on their passage through the front lens of the objective where the pencil is still diverging strongly, then from the point where the divergence is weak, or convergence commences, its lateral limitation, whether occasioned by a diaphragm actually situate at the place, or only conditioned by the previous course of the rays, must nevertheless produce a diffraction.

As regards the final result, it makes no difference whether the aperture at the circumference of the pencil of rays be supposed to be situate a little more to the front or to the back. The image of this aperture formed by the ocular lenses will be very slightly larger when it is situate at the back lens than when it lies in the front lens, but the difference is without any practical significance.

In equation [8] δ' is the breadth of fringe in the last image, a the divergence angle in the medium where the aperture lies, λ the wave-length at the same place, N the amplification of the last image, as distinguished from that formed by the rays passing the aperture.

If, on the other hand, we put N_1 for the amplification of the last image referring to the object λ_1, and n_1 for the wave-length, and refraction index for the medium in which the object lies, we may according to equation [7] make, as a is, by assumption, small,

$$\frac{n_1}{N_1} \sin. \alpha_1 = \frac{n}{N} . \alpha.$$

a_1 is the divergence angle in the first medium.

Putting the value of $\frac{a}{N}$ in equation [8], it becomes

$$\frac{\delta'}{N_1} = \tfrac{1}{2} \lambda \frac{n}{n_1} \cdot \frac{1}{\sin. \alpha_1} = \epsilon;$$

or, as $\lambda\, n = \lambda_1\, n_1 = \lambda_0\, n_0$, which last refers to air medium, we have

$$\frac{\delta'}{N_1} = \frac{\lambda_1}{2 \sin. \alpha_1} = \frac{\lambda_0}{2 \sin. \alpha_0} = \epsilon.$$

This ϵ is the true magnitude of those lengths in the object, which in the magnified image of the fringes appear equal, and will therefore be effaced. Therefore, ϵ may be considered the measure of the smallest distinguishable distances in the object. ϵ will be smallest when a_0 is largest,—that is to say, when amounting to a right angle. In that case

$$\epsilon = \tfrac{1}{2} \lambda_0. \qquad [9]$$

This determination of limit is likewise, as may be seen, independent of the construction of the optical instrument. It holds just as valid for a photographic apparatus as for the relation of the microscope to the eye of the observer. These are the formulæ which were applied in the calculations previously given.—*From the Proceedings of the Bristol Naturalists' Society*, New Series, vol. i. part 3.

PROGRESS OF MICROSCOPICAL SCIENCE.

Fusisporium Solani and its Resting Spores.—In reference to this fungus, Mr. W. G. Smith sends us the following quotation from an article which recently appeared in the 'Gardeners' Chronicle' from his pen:—"*Fusisporium Solani* is a fungus which very commonly occurs on diseased potatoes in company with *Peronospora infestans.* One is as destructive to the potato as the other; and Mr. Berkeley, writing of the former in 1857, describes it as a second enemy of the potato, 'equally destructive with the Peronospora, and, according as the two are separate or combined, different appearances arise. In some cases,' continues Mr. Berkeley, 'it produces an extreme degree of hardness, inducing a condition like that of the mummified silk-worms. Sometimes, on the contrary, it causes rapid and loathsome decay, especially when in company with the Peronospora.' Like the latter, it suddenly appears on the potato plant, carries on its work of destruction, and vanishes. Till now I believe the resting condition of *Fusisporium Solani* has never been described. In my attempt to work out the life history of *Peronospora infestans,* the undoubted resting spores of the Fusisporium came to light in the following manner:—A quantity of badly infected potato leaves were selected and isolated last July with the view of watching the Peronospora. As the presumed oospores of the latter gradually appeared, there also appeared much smaller bodies, which also went to rest; these were so similar in size and appearance to antheridia or dead zoospores, that they were thought to belong to one or the other. When I recently placed some of the presumed oospores of Peronospora in pure water to promote germination, all the smaller bodies at once burst, and in the short space of six hours developed into perfect plants of *Fusisporium Solani.* In size the spores measure about the $\frac{1}{2500}$ of an inch in diameter; they are palish brown in colour, with a very finely muricated outer coat, and a light central nucleus. The Fusisporium is frequently produced close to the resting spore, and I have observed the direct germination and production of the Fusisporium in innumerable instances. How these resting spores arose last year I am not certain, but it is not improbable that they may be a different condition of the aerial fruit broken up into four parts."

Microscopical Structure of Rocks.—This subject, which was almost unknown a few years ago, becomes now more popular every day. The latest investigation in this direction is that of M. Michel Lévy, which is reported in the 'Revue Scientifique,' and abstracted as follows by the 'Academy' (May 20):—In relation to acid rocks, it is observed that under the microscope they present the appearance of being composed of elements formed in succession at different epochs. The oldest crystalline elements are frequently broken, and worn or rounded, at their edges. They bear unmistakable marks of the mechanical actions that accompanied their eruption. They may be distinguished as ancient crystals, or crystals *en débris,* from the more

or less crystalline or amorphous magma by which they are surrounded, and which had its origin at the time of the consolidation of the eruptive rock. This distinction is well marked in porphyritic rocks. These rocks are generally composed of well-developed crystals imbedded in a more or less crystalline paste. This paste is the magma of consolidation, while the crystals are ancient. In ancient granites the crystalline elements of the magma of consolidation have dimensions comparable with those of the ancient crystals, so that it is difficult to distinguish them with the naked eye. The ancient crystals are black mica, amphibole, oligoclase, orthose and quartz, and the magma orthose and quartz. Recent quartz is moulded on earlier crystals; ancient quartz found in a mass that was still fluid exhibits bipyramidal grains. This form, which some geologists have considered characteristic of porphyries, merely indicates the presence of ancient crystals. The ancient crystals in granites with white mica, and elvans, are chiefly formed of black mica (not abundant), quartz, oligoclase, and orthose; the magma being orthose, quartz, and white mica. The white mica is the latest crystallized, from which it results that the recent quartz was often able to crystallize in its proper form, and thus, like the ancient quartz, exhibits bipyramidal grains. On the borders of massive granites with white mica, or when the rock is injected with thin veins, the magma is finer, and the texture porphyritic. This constitutes elvans, which appear under the microscope completely crystallized and formed of very small elements of quartz and white mica. In granulites the ancient crystals are rare, and the magma composed of united grains of felspar and quartz. In spots of certain dimensions the crystalline elements of felspar arrange themselves parallel to each other as if to form a more developed crystal. The ancient crystals of the porphyritic group do not serve to classify them. The magma, however, is sometimes entirely crystalline, as in granulites, while in the Permian porphyries it is more or less amorphous, and in optical properties approaches the vitreous rocks. These groups M. Lévy distinguishes as granulitic and petrosilicious porphyries. In granulitic porphyries he finds the ancient crystals composed of black mica, amphibole, pinite, quartz, oligoclase, and orthose. The magma closely resembles that of the granulites, but the elements are generally smaller. Frequently round the ancient crystals are mixtures of orthose and quartz, reproducing, on a small scale, graphic pegmatite. While it is difficult to establish sharp distinctions between different granulitic porphyries, the more recent are usually characterized by a finer microgranulite, and by a micropegmatite of smaller components. In the last rocks of the series, the micropegmatites only form aureoles, or fibrous radiating globules, difficult to resolve under the microscope; but the character of the orientation of the recent quartz is always dominant, and the aureoles and globules become extinct when the Nicol prisms are crossed. In petrosilicious porphyries (eurites of Grüner, Permian porphyries) the magma exhibits a more or less considerable proportion of amorphous paste, extinguished in all directions by the crossed prisms. They also present the texture called "fluid"—that is to say, the débris and the

impurities entangled in the paste are unequally distributed, and form more or less coloured zones analogous to those exhibited by matters suspended in a moving liquid. In this amorphous paste fibrous globules, like those of total extinction, are frequently observed, but the optical properties resulting from the radiated structure are dominant, and, instead of becoming extinct at a definite position of the prisms, these spheroliths exhibit in all positions a black cross in the direction of the principal planes of the prisms. At the close of the series of Permian porphyries the paste becomes entirely vitreous, and often presents the retreating cracks, roughly concentric, which characterize the perlitic texture. In the midst of this perlite paste the radiating spheroliths are often well developed, and a great part of the pyromerides ought to be grouped with the pitchstones. The acid rocks of the recent period present under the microscope a striking analogy with the ancient ones, but may be distinguished by the nature of their included crystals.

The Pollen of the Cherry.—Mr. A. W. Bennett writes a letter to 'Nature' of May 11 in explanation of the form of the pollen which he there figures. He states that he is compelled to do this from the fact that it is imperfectly drawn in the figures in Balfour's, Le Maout's, and Dr. Hooker's (primer) works. He states that "though somewhat variable in size and form, the grains are, I believe, never spherical, but ellipsoidal, with three longitudinal furrows."

The Circulation of the Sap.—In 'Flora,' * a German botanical journal of considerable interest, there is a review of Mr. Clark's lecture on the circulation of the sap in plants. An American journal, criticising it, says "the reviewer is discriminating, and points out some possible errors of interpretation, but appears to have thoroughly appreciated the wide range of experiments, and the energy with which the work was done."

Structure of the Leaves in Grasses.—In the 'Annales des Sciences Naturelles' † appears an elaborate article on this subject by M. J. Duval-Jouve. Criticising this, Professor Asa Gray says:—"Many of the text-books still say of the leaves of grasses, and indeed of Monocotyledons generally, that their veins or nerves are simple and unconnected by anastomosis; although what was meant must have been that the only anastomosis was by ultimate transverse veinlets. Duval-Jouve cites a long list of grasses in which these are conspicuous; and there are many in which the reticulating veinlets are of different orders. The stomata of grasses are in some confined to the lower surface of the leaf; in others divided between the two faces; in several they are restricted to the upper face, but in these the blade makes a turn or twist, so as for the most part to present this upper surface to the ground. *Triticum junceum*, *Calamrogastis* (*Psamma*) *arenaria*, and *Gynerium argenteum* (Pampas grass) are cited as instances. Many grasses have under the epidermis of their upper face, and sometimes of the lower also, rows or bands of large thin-walled cells, which our author names *bulliform* cells. These in their presence, absence,

* No. 29, 1875.

† Tome i. series 6.

number, and arrangement, are uniform in each species, but often quite different in the same genus, so that they may be used for critical specific characters; and they are, moreover, connected invariably with the vernation of the leaf, and with the opening and closing (either by conduplication or convolution, according to the vernation of the species) which are so prompt in many grasses. That this movement takes place in virtue of the hygrometric expansion of these cells under moisture and their contraction in dryness, was made plain by the behaviour of sections of the leaf under the microscope, the closed conduplicate leaf of Sesleria opening instantly upon the application of a drop of water, when these cells in a band on each side of the midrib, before flattened or collapsed, became turgid and prominent. The leaves of *Leersia orvyzoides* are described as rolling up instantly upon being bruised or roughly handled, as if endowed with real irritability. We trust some of our young botanists will look to this, next summer. The split sheath of the leaves is one of the diagnostic characters of the Gramineæ. Exceptions in Glyceria, &c., were familiar. M. Duval-Jouve states that about a fifth part of the species have entire sheaths. Also that various grasses bear two, three, and even four leaves on one node!"

Microscopic Characters of Inflammation.—These have been admirably explained by Dr. Burdon Sanderson in his recent lectures on the subject. He said that it was in the batrachian eye the process may be most advantageously studied by touching the very centre of the corneal surface carefully with an extremely fine point of nitrate of silver. If the cornea so acted upon is prepared thirty-six hours afterwards with the aid of chloride of gold, it exhibits to the naked eye a central eschar surrounded by an area which can be distinguished from the rest by its somewhat brighter colour; and if the tissue of this area is examined microscopically, it is found that within it the cornea corpuscles present an altered and, as it were, shrivelled appearance, so that the network of protoplasm, with the appearance of which everyone is familiar in the normal cornea, has lost its continuity and integrity, while in the neighbourhood of their nuclei many of the corpuscles exhibit little holes, as if containing clear liquid, excavated in the protoplasm. From the presence of these holes—commonly spoken of as vacuoles—the zone may be called the zone of vacuolation. The appearances seem plainly to indicate that the caustic has, in the immediate neighbourhood of the eschar, injured or disintegrated the tissue by its direct action. What follows may be seen in preparations made some twenty-four or thirty-six hours later. By this time exudation has begun from the border of the cornea towards the injured part. *By infection* the vessels of the limbus have become congested, so that the conditions favourable for the sweating out of liquor sanguinis and the escape of leucocytes are present. Soon protoplasmic masses, differing entirely from those proper to the corneal tissue, present themselves among the normal elements, which, from the form they assume and their distribution, appear to be making their way centripetally. It is at this point that the question which it chiefly concerns us to answer presents itself. Ten years ago, if we

had examined a cornea at this stage, we should have said without hesitation, Here is an instance of abundant "proliferation" or germination of tissue; the cornea corpuscles have given birth *in situ* to a progeny much more numerous than themselves. At that time the modes of preparation in use were very imperfect and unsatisfactory, so that a great deal that is now clear was necessarily left to conjecture. Appearances were seen under the microscope which suggested endogenous multiplication of cells, and there was no reason for doubting that these appearances corresponded to the reality. Now we are in a somewhat different position. Cohnheim's discovery of the chloride of gold method (to which others nearly as valuable have since been added) has placed both supporters and opponents of germination in a far better position than before. The result has been unquestionably unfavourable to the continued admission of the doctrine of the textural origin of pus. For the position of matters is such that the attentive study of the preparation carries to the mind the conviction, in spite of previously conceived opinions, that the relation of the adventitious corpuscles, with which the tissue of the inflamed cornea is beset, to the normal elements, is one of juxtaposition merely, the convincing fact being that, even in those parts of the cornea where the immigrant corpuscles are most numerous, the proper cornea corpuscles are still seen to present their normal aspect and distribution.

Structure of the Genus Brisinga.—M. G. O. Sars, the celebrated Norwegian naturalist, has published a memoir on the anatomy of this genus, founded on an examination of a new species, *B. coronata.* In this valuable memoir, which is illustrated by seven excellent plates, Professor Sars has given a detailed description of the anatomy, physiology, and development of the genus Brisinga, perhaps the most remarkable form of star-fish hitherto discovered. The author also discusses, at considerable length, its relations to other star-fishes, recent and fossil, as well as to Echinoderms in general, and the relation of Echinoderms to the Annelids. He regards Brisinga as the most generalized form of star-fish, and consequently of Echinoderms, and supposes it to be one of the little-modified survivors of a primitive type from which the other forms of Echinoderms have descended. It has affinities to the most ancient fossil star-fishes of the Palæozoic rocks (Protaster, &c.). The existence of a genuine vascular system, distinct from the general perivisceral cavity and its extensions, is denied both in the case of this genus and of other star-fishes. The author also states that there is no anal orifice, although there is, as in other star-fishes, a dorsal gland, with a narrow duct opening on the dorsal surface, and he suggests that this duct has in other star-fishes been mistaken for an intestine, and its outlet for an anus, the existence of which, in any star-fish, he doubts. Professor Sars adopts the view, previously advanced by Duvernoy, Huxley, and Haeckel, that an Echinoderm is a cluster (or "comus") composed of several articulated zooids ("persons") united by their anterior ends.

Development of Unfecundated Ovules of the Frog.—Several observers have noted the occasional partial development of ovules

which have not been exposed to the possibility of fecundation. The fact was mentioned by Bischoff and R. Leuckart, and M. Moquin-Tandon has recently communicated some analogous and more detailed observations to the Académie des Sciences. The first phases of segmentation were distinctly observed in the egg dropped by a female frog which had been kept in confinement for about four months, and secluded from all possible intercourse with the male. In the ovule, first two large vertical fissures were seen, and then a horizontal fissure. The segmentation then proceeded in a less orderly way, the vitelline spheres multiplying irregularly, and becoming of unequal size. The process was more rapid than in fecundated eggs which were allowed to develop at the same temperature. Only a small number of the ova presented this evidence of commencing development; the majority died without any sign of segmentation. In all cases the phenomena soon ceased, the spherules produced separated, and the whole mass began to decompose. Sometimes death occurred after the division into two or four segments, sometimes at a more advanced period, but the ovule never assumed the mulberry look. M. Moquin-Tandon points out that the observation establishes incontestably that the ova of vertebrata not impregnated by spermatozoa may pass through the earliest stage of development in certain conditions, the exact nature of which is at present unknown. These facts, says the 'Lancet,' may be placed beside those of the same kind observed by Bischoff on the sow, by Hensen on the rabbit, by Agassiz and Burnette on fish, and especially with the remarkable fact observed by Oellacher that in fowls kept far from a cock unfecundated eggs underwent segmentation in the interior of the oviduct.

NOTES AND MEMORANDA.

American Photographs of Blood-disks.—Mr. G. Gulliver, F.R.S., who is known to be the greatest living authority on the subject of blood-corpuscles, has sent us the following note. We may mention that we ourselves have received a photograph from Dr. Richardson, of America, which, as mentioned in a former number, exhibits the difference between the blood of man and the pig with great distinctness. " Through the courtesy of Professor Thomas G. Wormley and Dr. G. Richardson, I have received several photographs of mammalian red blood-corpuscles compared with those of man. In each specimen, on a single slide, the corpuscles of the human subject are shown in one microscopic field of vision with those of brutes. Thus the human blood-disks are compared with those of the pig, cat, dog, &c., and in a very instructive manner; especially as a group of such minute bodies displays better their differences of size in different species than a single corpuscle of each of them. I know not that such preparations of these corpuscles have been made in Europe, but anyhow these are very creditable to American microscopy. Objection might be taken

to the crowding and irregularity of shape of the pig's and human corpuscles on one slide; but this very fact is valuable, since it shows well the softness of those of mammalia, and their proneness to become misshapen by mutual pressure. Hence this photograph, duly enlarged, would be instructive as a diagram for lectures. Some of the others are admirably done, with the corpuscles more or less separate: the slide showing those of the dog and man is quite true to nature—as indeed are all the preparations; and it is to be hoped that American physiologists will continue these valuable representations, which so well exhibit the form and comparative sizes of the blood-disks; especially as this subject, though full of importance and physiological significance, seems to be almost ignored in Europe. Dr. Richardson, too, has insisted on the medico-legal value of the sizes of the corpuscles, and on this point it is understood that he is still prosecuting his original investigations; in connection with which there should be preparations of the membranous bases of the mammalian red corpuscles, side by side, on the same slide, with the entire corpuscles; and then, whatever may be the exact constitution of them while circulating in the vessels of the living body, how can it be maintained, according to a modern view, that, out of the body, as we examine and wash away with water the greater part of their bulk, leaving the membranous bases behind, the corpuscles are mere homogeneous bodies, like minute drops of oil or treacle?"

New Microscopic Amplifiers.—The Rev. J. H. Wythe, M.D., exhibited two instruments of the above kind at a meeting of the San Francisco Microscopical Society, held on March 16. He observed:—"From the great improvements in object-glasses, made within the last few years, it would be reasonable to infer that opticians have reached the limit of perfection in that direction, and that future progress in the power of the microscope must depend mainly upon the eye-piece, or intermediate arrangements of lenses between the eye-piece and object-glass. A conviction of the possibility of improvement in this way has led me to many experiments during the last two or three years, and has resulted in the discovery of the amplifiers herein described, by which the magnifying power of an objective and eye-piece may be increased fourfold or greater, without apparent loss of definition. In the recent edition of 'Carpenter on the Microscope' (1875), the only means of amplification suggested are the employment of deep or strong eye-pieces, and the use of the draw-tube. The aplanatic searcher of Dr. Royston-Pigott (described in the 'Microscopical Journal') is referred to as an amplifier; but I have no experience in its use. The meniscus is said, by one of the journals, to have been used as an amplifier; but I have seen no description of it—the article to which I refer omitting to state whether it is a convex or concave meniscus, or how it is used. The above are all the suggestions I have found in microscopic literature. Experimenting upon the suggestions, I arranged the strongly magnifying eye-piece, which I exhibited to the Society upon a previous occasion, consisting of a deep convex meniscus, in place of the ordinary field-lens in the Huygenian eye-piece. This, tested upon the *Pleurosigma angulatum*, &c., gave excel-

lent results. Further experiments have led to the employment of the two amplifiers I now describe. Either of them is used in a sliding tube between the eye-piece and objective, and the proper position is found by trial. The first consists of a cylindrical lens, conical in shape, with the smaller end concave, toward the object-glass, and the larger end convex. This gives a large increase of magnifying power and excellent definition when used with the strongest eye-piece of Gundlach, or other makers. The second form is better still, and consists of a double concave lens, having a virtual focus of about $1\frac{1}{2}$ inch, at the end of a tube about 6 inches long, at the other end of which is the ordinary negative eye-piece. In both these forms the extent and flatness of the field is quite remarkable, as well as the amount of light, while the amplification is very great. With a periscopic eye-piece of Gundlach, or the No. 3 of the same maker, or with the strongest eye-piece of Crouch, my $\frac{1}{8}$th objective defines the semi-lenses on the frustule of *Pleurosigma angulatum*, the markings on *S. gemma*, or *Grammatophora subtilissima*, with a power of 4000 diameters."

The Microscopes in the Loan Collection at South Kensington.—We are sorry to confess our disappointment with this exhibition, but we are bound to confess that it has disappointed us; firstly, because the collection of instruments is so meagre—the Royal Microscopical Society being alone well represented; secondly, because it is badly arranged; and thirdly, because there is an utter absence, save in one or two instances, of explanatory cards attached to the instruments. We had hoped that the subject which of all others has created most discussion in the microscopical world—the immersion system—would have received particular attention; but we have not found that any attempt has been made to illustrate it. Then there is an utter absence of that chronological succession which would have been useful to the ordinary student as well as to the histologist. Indeed, almost the only novelties that we have observed are the moist chamber recently described before the Royal Microscopical Society by Messrs. Dallinger and Drysdale, and the contrivance exhibited by Professor Klebs for studying the multiplication of the *Schizomycetes.* Of the earlier instruments are those—some of which are familiar enough—of Martin, Janssen, Lyonet, and others. An instrument from the Geneva Association is also of interest, from the manner in which the object-glasses are attached, the arrangement being one of spring-catches, which admits of the objective being removed and replaced with considerable rapidity. We doubt, however, whether the centering of such glasses would be at all perfect.

Of the instruments exhibited, the following are among the more worthy of notice. A solar microscope, from the Essex Museum; Goring's aplanatic engiscope; an interesting case of Lieberkühn's preparations, lent by the Royal Museum of Cassel; instruments by Culpepper, Amici, and Marshal. Dr. Royston-Pigott exhibits two very interesting instruments, which we wish had been better placed; one is his kratometer for finding the magnifying power of objectives, and the other his microscopic refractometer for ascertaining the refractive indices of plates of glass. Among M. Nachet's exhibits may be

specially mentioned an instrument for the taking of microscopic photographs. The most novel of the things exhibited are Mr. Dallinger's improvement, in the one case in the apparatus for securing continuous observations of organisms, and in the other his ingenious contrivance for securing perfectly central illumination, both of which have been described in this Journal. Strangely enough, the largest collection of instruments is made by a private gentleman, Mr. Crisp, F.R.M.S. This is a most typical group, and consists of many splendid examples of the various types of instrument in present use. Among the more remarkable in Mr. Crisp's series is one called Brown's pocket microscope. There are also microscopes exhibiting Wenham's improvements. Mr. Stephenson's well-known binocular and erecting microscope. Chevalier Ross, Beck, and How also have instruments exhibited.

It will hardly be believed that there is no catalogue for the museum, yet such is absolutely the case. We know not who is to blame for this; but assuredly some one's back must be broad enough to bear it. There is a very large handbook to be purchased for a shilling, in which we found nothing whatever in the shape of a catalogue. In it, however, we did find a couple of articles, one by Professor Huxley, and the other by Mr. Sorby, which are well worthy of perusal by our readers. Mr. Sorby gives an admirable sketch of the microscope as it is, and we think one paragraph is particularly worthy of consideration. It is that in which, after describing the mode of making an object-glass, and correcting for spherical aberration, he says: "The attainment of all these advantages is so extremely difficult in the case of high powers, that even the best object-glasses are little more than the best possible compromises between opposing qualities, and *it becomes a question whether lenses of high power should not be designed and made each for a particular class of objects, since a quality which is of paramount importance in one case is not in another.*" The italics are ours. We think this passage particularly suggestive to the makers; for there is no doubt that what the histologist requires is a glass of quite different qualities from that demanded by the diatom observer. We shall conclude these remarks by a quotation from Professor Huxley's observations. The Professor says:

"It is true that the very interesting collection of ancient and modern microscopes in the Collection contains a compound microscope, invented and constructed about the year 1590 (No. 3513), but it is little more than a toy. The seventeenth century hands down to us the microscopes of Leuwenhoek (3512), venerable relics of the epoch at which the foundations of minute anatomy were laid; while that of Lyonet (3525) reminds us that the eighteenth century saw the production of one of the most perfect pieces of minute dissection yet extant—the 'Traité anatomique de la Chenille qui ronge le Bois de Saule.'

"In the hands of Malpighi, Leuwenhoek, Grew, Swammerdam, Lyonet, Hewson, and others, the simple microscope, either as a single lens, or in the doublet or triplet form, did wonders; while Ruysch's exquisite methods of injection showed how the difficulty, not to say

impossibility, of tracing out the more minute vessels and ducts of organized structures by mere dissection could be overcome.

"Dissection, aided by maceration; microscopic investigation, carried as far as the simple microscope would go, and doubtfully assisted by the imperfect earlier forms of compound microscope; and injection, by the syringe or by the mere weight of mercury,—remained the sole methods of anatomical research up to within the last fifty years.

"The improvement of the compound microscope in the early part of this century (see No. 3526), by the invention of adequate methods of correcting spherical and chromatic aberration, and of illuminating objects, has enabled anatomists to extend their investigations into minute structure to an unhoped-for degree, and to use magnifying powers of 2000 to 3000 diameters with as much confidence as was placed in those of a fourth that amount forty years ago."

CORRESPONDENCE.

Hasert's Objective System.*

To the Editor of the 'Monthly Microscopical Journal.'

Darmstadt, *May* 16, 1876.

Sir,—In No. lxxxvi. of your esteemed Journal, which reached me only recently, M. Hasert, of Eisenach, has tried by his usual means to put (suitably to his purpose) in a questionable light my impartiality and love of truth, notwithstanding that I never since the appearing of the first volume of my book, 'The Microscope,' viz. since 1867, met anywhere with any objections of his to the results of my investigations before.

It is far from my intention to enter into any discussion with this gentleman, who is well known to us German microscopists, but I think it due to the readers of your Journal, few of whom may be acquainted with my book, to state *the truth* of the matter.

To this end I need only repeat the passages in question concerning the "Hasert's objective" occurring in my book, 'The Microscope,' appending a few short remarks, for which I beg you will kindly grant me a little corner in your esteemed Journal.

Vol. i. p. 119, treating of the drawing of the butterfly scales, it is stated, "Bruno Hasert has discovered this structure in 1847, and more fully treated this subject since (Official Report of the Meeting of the German Naturalists and Physicians in Carlsruhe, page 212),

* Herr Dippel's letter has unfortunately been sent to us in the original German, which is most complex in its mode of expression; however, it has been, considering the peculiarity of the style, successfully translated by our correspondent W. R.—Ed. 'M. M. J.'

and at his instance I have more specially examined them with the aid of my strongest 'objective systems,' and fully convinced myself of the correctness of his views. An unmistakable position has thus been gained as to how the diagonal striæ may be seen with good instruments."

Now although the above words do contain an indirect praise of Hasert's systems of 1861, it is surely not averred that they are the best, or unsurpassed for *every* investigation, neither is it said that *my own* examinations were made with the aid of *his* glasses, which in fact they were *not;* at the most it was shown that "Hasert's objectives" in question were, in a certain sense, superior to the older ones with a smaller angle of aperture.

On page 143, treating of the colouring of the "field of vision," it is said, "I have always found the yellowish colouring to be most strongly developed in various gradations, and it becomes, for instance, with some of Hasert's objective systems (here produced by the glass, or rather the liquid enclosed in the lower lens) so strong, that it makes the whole field of vision resemble a sheet of white paper covered with a thin layer of gum, and positively becomes troublesome to the eye, and obscures some more delicate shadings, '*and I hold this prominent colour to be a most serious fault* OF ANY OBJECTIVE SYSTEM, *rendering it almost useless for the practical microscopist, even if in other respects* IT ANSWERS WELL.'"

This expresses, no doubt, most decided censure, but every microscopist who ever had objectives of Hasert's of that period will, I feel sure, agree that it is deserved (vide opinion of H. von Mohl and Professor Schacht).

On page 169 it is said, with reference to Hasert's strongest objective $\frac{1}{14}$ inch, "On Nobert's plate are solved the 18th, and if illumed obliquely the 26th group." (The discrepancy between the results as published by me and contained in my letter, is explained by the fact of my having used the clearest white cloud light ("hellstem weissen Wolkenlichte") at the first trial; whilst the comparative trial afterwards was made with a clouded sky and the careful shutting off of all side lights.) In the same manner will "central" illumination solve *Pleurosigma angulatum*, *Grammatophora marina*, and *Navicula veneta;* and "oblique" illumination all natural test-objects without showing the same full clearness as is obtained with Hartnack's systems, 9, 10, and 11; and summing up then the total result of my most elaborate and comprehensive trials, I said here, "It follows thus, that the objective systems of Hasert, although mechanically most simply and strangely, not to say carelessly, constructed, *occupy a very high rank as regards their solving power, and might also be said to rival the immersion lenses of Hartnack and Amici, but are much inferior as regards beauty and clearness of the picture under central illumination.* For those microscopists who principally study the Diatomaceæ (here I may be justified in referring to Dr. Schumann's researches) they are really excellent, and sure to excite the admiration of those who hold the solution of the Grammatophora and similar objects as the masterpiece

of a system. But for the practical microscopist (meaning especially the histologist) they have many faults which restrict their use. For instance, in all the three powerful systems the distance from the object is exceedingly small, and they demand therefore far thinner cover-glasses than the equally powerful systems of Hartnack, Zeiss, and Belthle. Besides, the picture obtained with the usual mirror illumination is, scientifically speaking, of less use. Placing, however, the achromatic condensing lens between mirror and object, and regulating its position carefully, the pictures of the organic objects assume a totally different appearance, with clear outlines and without coloured edges. (To this refers my second epistolary remark following an equally epistolary reproof on the subject.) Finally are these glasses very deficient as regards 'light,' and require it direct from the sun (this was specially mentioned in Hasert's letter after my letter finding fault with it), which is a great obstacle to their use. Most decidedly, too, I must blame the obnoxious yellowish colour of the 'fields of vision,' which renders the use of them distasteful."

These words, I think, are straightforward and denote "fair play," and are a proof that praise and censure are bestowed where merited, and exactly in the same place M. Hasert will find them in my letters. I dare say it may interest your readers to inquire how many of our noted histologists are using Hasert's objectives. Finally, as regards the "neatly told story" of M. Hasert of the occurrence at the meeting of the naturalists.* The following remark on the above-cited page will, I think, deal with it effectually, for it says: "'I for my part cannot *unconditionally*' agree with the praise lately ('Botan. Zeitg.' 1863, No. 10) bestowed by Professor Hofmeister on the instruments by Hasert." The "why" is easily explained by the faults just censured; besides, I think it my duty to advise people not to be led implicitly by the stylish advertisements of Prof. Hasert; for instance, page 124, 'Botan. Zeitg.' 1864, says: "*The 'correction' for the different thicknesses of the covering glass is no longer required; trials with single, double, or triple thickness of the glass yield equally good results.*" When I, however, at the meeting in Giessen (autumn, 1864) sought by means of a "Pine section" to try such an objective system *insensible to thickness of covering glass,* I found that the latter, under 0·1 mm.,† although perfectly of avail for the most powerful *systems of Hartnack* 10 ($\frac{1}{16}$ inch engl) and 11 ($\frac{1}{18}$ inch engl) became useless.

I hardly know for what purposes any practical microscopist could use such a system."

I believe the above is sufficient to prove that M. Hasert's state-

* How, for instance, does this good gentleman know that the covering glass of my preparation measured at least 0·75 mm., as he had neither measured it, nor could do so at the time? I suppose I need not tell you that I don't use for such objects a covering glass of looking-glass materials.

† Exact thickness I measured 0·08 mm. The preparation is up to this day in my possession, and has ever since done its duty with the systems of Gundlach IX., 1868; Seibert IX., 1873; Benèché XII., 1869,—all three equal to $\frac{1}{32}$ inch engl. Zeiss' immersion No. 3 = $\frac{1}{25}$ inch engl, without the glass cover having been found too thick.

ment, page 95, is, to say the least, rather coloured and fanciful, and that I was perfectly right, indignantly to turn my back to him after such a result, he even trying to abuse M. Hartnack and others.

Yours most devotedly,

DR. LEOPOLD DIPPEL,
Professor in Ordinary of Botany.

ANGLE OF APERTURE.

To the Editor of the 'Monthly Microscopical Journal.'

SIR,—In anticipation of the reformed list of apertures of microscope objectives that I believe will be required, I may recommend microscopists, as an adjunct with the sector, to try their present apertures by means of a micrometer measurement of the diameter of the luminous spot, or working portion of the front lens; taking this as a base, and the distance of the focus as a height, for the angle. This is easily accomplished with the ordinary means at hand.

It seems a forlorn act for Mr. Tolles to ignore my measurements, and send others across the Atlantic in place of them. His case has at length collapsed. I had not before the last revival of the question that confirmed this conclusion measured the portion of the true *working* diameter of the front lens (·033) or the exact distance of the immersed focus (·025) corrected for an object actually *mounted in balsam.* These measurements I can show so plainly and unmistakably, that no unprejudiced person can for one moment doubt them.

I am, Sir, yours truly,

F. H. WENHAM.

PROCEEDINGS OF SOCIETIES.

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *June* 7, 1876.

In the absence of the President and Vice-Presidents, the chair was taken by Mr. H. J. Slack, on the motion of Mr. J. W. Stephenson, seconded by Mr. J. C. Sigsworth.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society was read, and the thanks of the meeting were voted to the donors.

The Chairman called attention to a donation from Mr. Hanks, of San Francisco, consisting of some diatomaceous earth from Santa Monica, near Los Angelos, and which seemed to be very rich in various circular forms. Some slides of diatoms from this material had been prepared by Mr. Barnett, and were upon the table for examination.

The Chairman said that most of the Fellows were aware that the Society was constantly indebted to Mr. Kitton for his kindness in examining and naming diatoms which they submitted to him, and it had been thought by the Council that his services should be in some way recognized by the Society. They had therefore proposed that Mr. Kitton should be nominated for election as an Honorary Fellow of the Society. The resolution of the Council to that effect was then put to the meeting and unanimously confirmed, and Mr. Kitton's name was ordered to be suspended in the usual way.

The Chairman then read to the meeting the translation of a paper "On a Photograph of Nobert's Bands," which had been addressed to the President, and was communicated to them at his request. The paper was written in Italian by the Abbé Count Castracane, and some further remarks upon the subject by the President were appended to it. (The paper will be found at p. 6.)

The thanks of the Society were unanimously voted to the Abbé Count Castracane and to the President for their communications.

Mr. Henry Davis gave an abstract of a paper "On the Formation of Conochilus," and illustrated his remarks by numerous drawings on the board showing the differences between that genus and the Floscules, and also the results of his own observations which had led to some very interesting additions to the knowledge of the life history of these rotifers. (The paper will be found at p. 1.)

The Chairman felt sure that all who were present would return their hearty thanks to Mr. Davis for his interesting communication. He could quite confirm what Mr. Davis had said with respect to the tails of these creatures; but he should like to ask at what particular season of the year the males were found, and also at what time the "winter eggs" abounded in the gelatinous mass?

Mr. Davis said he found the male rotifers in July and August 1875; but he fancied that Dr. Hudson had found them recently. The winter eggs might be found in every month of the year, so that there was really no ground for applying this name to them. He thought they served the purpose of resisting the effect of drying up the ponds in a season of drought, just in the same way that the gelatinous coverings of some other creatures had been shown to do in a paper which he read to the Society some time ago.

The Chairman supposed that if only a small number of these winter eggs hatched, it would not be enough for the formation of a group, and inquired how few a number Mr. Davis had found grouped together.

Mr. Davis said he had found as small a number as six in one group, but he had observed that when only a small number were found together, the individuals were all of the same age, whereas when a large group was examined it would be found to consist of individuals of all ages.

The Chairman was quite sure that they all appreciated Mr. Davis's new and important discoveries, but the fact of their being new was of course the reason why no one had at present much to say about them.

The thanks of the meeting were voted to Mr. Davis for his paper.

The Chairman said he had brought for the examination of the Fellows a specimen of petalody of the sepals of Gloxinia. Two portions of the cleft sepal instead of preserving their pointed shape had expanded and assumed the colour of the corolla.

The Chairman again called the attention of the meeting to the diatoms exhibited in the room, which he thought would be found worth notice. Diatoms he thought might be broadly divided into two groups, one of which had more or less distinct rows of beads, and in which a fracture always occurred between the rows; and the other group might be described as belonging to the same type as the Coscinodiscus described by Mr. Stephenson and Mr. Stewart, and were composed of a beaded framework with areolar depressions of different sizes. If one of the latter type were examined—say, for instance, *Euphodiscus Argus*—with a good $\frac{1}{8}$-inch objective, not too wide in angle of aperture, it would be found to be composed of a framework which was distinctly beaded, and which enclosed areolæ. In some of the Californian forms on the table the fracture had taken place exactly in the way which Mr. Stephenson had indicated. He was inclined to think that the outer layer was in many cases perforated so that the water could get through to the layer below, at the same time it was very likely liquids could to some extent pass through a thin film of silica, and that a great deal of the nutrition might be taken in in that way.

Mr. Charles Stewart said there could be no doubt whatever that silex really was penetrated by water; that which was deposited in the cavity of the bamboo was clearly so, and the same might be shown with respect to that which was found in the common cane. In the latter the cells presented a peculiar appearance when looked down upon, and showed a more or less pear-shaped cavity completely covered with silicious matter.

Mr. Stewart then proceeded to describe two objects which he had brought for exhibition. The spines of the Echinodermata were for the most part very well known; but there were some which he thought were much less known than the others, namely, those which were found only on a limited portion of the surface of the shell immediately surrounding the pentagonal opening on the under side, through which the teeth protruded. These spines were found on all the common Echini except the Cidaridæ; in size they were extremely minute, and seen as opaque objects they glittered like little diamonds, and in form they were shaped more or less like water bottles. The main mass of their structure was formed of solid calcareous matter deposited layer by layer; but it did not show any indication of a black cross under polarized light, as might naturally be expected, but simply polarized of uniform dull colour, showing that the actual tension was in one direction only; and it was in reality as if a person had taken a piece of carbonate of lime and carved it into the shape of the spine. They had been spoken of by Louvain as the Spheridia. They were so few in number on some Mediterranean species as not to exceed five, though on the common British Echini as many as thirty and upwards might be found.

In addition to these spines, he had also brought to the meeting

another specimen of some interest, a section of the lachrymal gland of the common turtle, which will be described in the next number of this Journal.

The Chairman then moved the thanks of the meeting to Mr. Stewart for his communication. He also called attention to a discovery made by Becquerel affecting cell structures, namely, that whenever an animal membrane was moistened on both sides by diverse fluids, an electrical current was set up, and that this electric action was accompanied by electro-chemical decomposition.

The thanks of the meeting were unanimously voted to Mr. Charles Stewart for his communication.

The Chairman read the translation of a letter from Count Castracane, thanking the Society for the honour of being elected a Foreign Honorary Fellow. He then announced that, according to their usual custom, the meetings of the Society would be adjourned until the first Wednesday in October, and that the Society's rooms would be closed during the month of August.

Donations to the Library since May 3, 1876:

	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Journals of the Linnean Society. Nos. 63 and 83	*Ditto.*
Quarterly Journal of the Geological Society. No. 126	*Ditto.*
Proceedings of the Bristol Naturalists' Society. Vol. I. Part 2 ..	*Ditto.*
Monthly Notices of the Royal Society of Tasmania, 1874	*Ditto.*
Bulletin de la Société Royal de Botanique de Belgique	*Ditto.*

Major Fowkes and the Rev. C. Darby were elected Fellows of the Society.

WALTER W. REEVES,
Assist.-Secretary.

MEDICAL MICROSCOPICAL SOCIETY.

Friday, May 19, 1876.—Dr. F. Payne, President, in the chair.

Scirrhosis of Liver in a Child.—Mr. Needham communicated the particulars of a case, and showed specimens both of the liver and other organs. The child, æt. six years, was admitted as a patient in the North-Eastern Hospital, Hackney Road, on June 23, 1875. Her antecedents were good, and the child's health had never suffered till lately, when she was observed to be losing flesh, and six days before admission her abdomen began to swell. She had always been well taken care of, and had never drunk spirits in any form. In July the abdomen was tapped, but soon again refilled. 42 oz. of fluid were removed. The child rallied and returned home, but was readmitted in December, 1875, much worse, and suffering from attacks of epistaxis and hæmatemesis; and in January, 1876, she died in convulsions, after having vomited a quantity of coffee-ground material.

Post-mortem examination showed the liver to be "hobnailed," indurated, and weighing 19½ oz. It was uniformly straw coloured, no puckering of Glisson's capsule, and a section appeared fleshy,

intersected with bands of a tough material. Spleen firm; 6 oz. in weight. Other organs healthy, but anæmic. Stomach contained food and streaks of blood; but the source of the hæmatemesis could not be found.

A section of the liver showed with the microscope liver structure cut off into island by bands of connective tissue, in which were elongated cells that readily were stained with carmine; and besides these, dotted about, and easily stained, were very many round cells, like white blood-corpuscles, to which they seemed more related than to the long cells above mentioned. The liver-cells were often confused by this new growth, and were undergoing fatty degeneration in some places. Glisson's capsule was healthy, and had taken no part in the process. The kidneys showed proliferation of connective tissue, as did also the spleen; and between the gastric tubes in the stomach was much of the small cell growth, such as was seen in the liver.

The various organs had been prepared with chromate of ammonium and also with spirit, and had been stained in various ways; but the same appearances always presented themselves. In between the muscular fibres of the heart, and in the brain, especially about the blood-vessels, the same small cell growth could be well seen.

The President had never heard of a case where the changes were so universal. Similar conditions in the stomach and kidney had been before described, but never, as far as he was aware, in the heart; it would be extremely interesting if scirrhosis of the liver could be made a general disease. He had also seen a case of scirrhosis of the liver at the age of four years.

Dr. Pritchard suggested the presence of the small cells in the various organs pointed only to some irritation of the blood-vessels, and to no particular diseased condition.

The President inquired if the cells were found equally around arteries and veins; for if so, it was against simple inflammation being the cause.

Mr. Needham, in reply, stated that in another case of scirrhosis that he had examined he had found all the changes here described. The proliferation of small cells was equally around arteries and veins.

Quekett Microscopical Club.

Ordinary Meeting, April 28, 1876.—Dr. Matthews, F.R.M.S., President, in the chair.

The Secretary read an abstract of a paper, by Mr. W. K. Bridgman, "On the Principles of Illumination." The practical bearing of the paper was to advocate the use of an illuminating pencil directed upon the object at the polarizing angle, by which it was considered that the greatest amount of light and the least amount of glare were secured. The object, if transparent, was to be viewed in the line of the reflected ray; if opaque, in a direction at right angles to the plane of reflexion. The principle was applied to the stereoscope, and also to Lieberkühn illumination with the microscope, and it was shown how, by the management of the mirror and Lieberkühn, a pencil of

rays at the desired angle could be obtained. The paper was illustrated by diagrams showing the methods of procedure in the various cases.

Ordinary Meeting, May 26, 1876.—T. Charters White, Esq., M.R.C.S., Vice-President, in the chair.

Mr. Curties read a communication from Professor H. L. Smith, of Hobart College, New York, "On a New Method of Mounting Microscopical Objects." The process was applicable to either opaque or transparent dry objects, requiring cells. For mounting an opaque object, a circular disk of black or dark-coloured wax, of the kind used in the manufacture of wax flowers, was punched out to form the bottom, while the wall of the cell was made of a ring of brass wire of suitable diameter and substance, imbedded in the wax, which was attached to the slide by gentle heat. The cell was finished with Brunswick black, on a turn-table. Objects, such as Foraminifera, could be attached in any position to the wax bottom by softening it with a minute drop of turpentine. The cover, which was to be a little smaller than the ring, so as to be flush with the top when pressed down, could then be put on, and the cell finished with Brunswick black without any danger of its running in. For transparent objects, rings of wax were used instead of disks, the method of proceeding being the same. Minute details of each stage of the process were given, and specimens presented to the club.

Mr. Ingpen described an old and very interesting microscope by Amici, which was formerly in the Stow Collection of the Duke of Buckingham. The form was that afterwards adopted by Chevalier, and better known by his name, in which the rays are bent at right angles by a prism. The instrument possessed a micrometric stage, an achromatic objective, and various pieces of apparatus, and its workmanship was much admired.

A communication from Dr. Frances Elizabeth Hoggan, "On a New Process of Histological Staining," was read by the Secretary. This process was applicable to tissues either fresh, frozen, or hardened by picric acid or alcohol, but not by chloride of gold or any chromate. The agents used were a 2 per cent. solution of perchloride of iron, and a solution of pyrogallic acid of similar strength, in distilled water, or, preferably, in alcohol. The sections were to be first treated for one or two minutes with alcohol, then for about the same time with the iron solution, and finally with the acid solution, until the required depth of tint was obtained, when they could be mounted in balsam, glycerine, or varnish. It was recommended to filter the solutions at the time of use. Nuclei and nucleoli were coloured black by this process, rendering them very distinct when examined by lamplight; the cell substance was also coloured more or less, according to the age and other conditions of the tissues. The colour could be rendered bluish by washing in slightly alkaline water. The process could be used in conjunction with silver staining, and was deemed particularly suitable for photographic purposes. It had also the advantage of being very rapid, only occupying a few minutes. Specimens mounted in glycerine and in copal varnish were exhibited at the meeting.

South London Microscopical and Natural History Club.

An ordinary meeting of the above club was held on Tuesday evening, January 18, at the Angell Town Institution, Brixton. Charles Stewart, Esq., M.R.C.S., F.L.S., President, in the chair.

An address was delivered by W. M. Ord, Esq., M.D., F.R.C.P., on "Some of the Physical Conditions which determine the Forms of Minute Bodies."

At the ordinary meeting held on Tuesday evening, February 15, Robert Braithwaite, Esq., M.D., F.L.S., Vice-President, in the chair, a paper on "The Structure and Classification of some of the higher *Fungi*" was read by T. Howse, Esq., F.L.S. The lecturer restricted his remarks to the family *Hymenomycetes*, and after describing in detail the characteristics of the orders of which the common mushroom is a type, and the researches of Mr. Worthington Smith into the fertilization and reproduction of these fungi, Mr. Howse gave an account of the six orders into which this family is divided, devoting especial attention to the subdivisions of the genus *Agaricus* into five sub-genera according to the colour of the spores, each of these being again divided into three groups according to the structure of the *hymenium*. The other orders *Polyporei*, *Hydnei*, *Auricularini*, *Clavati*, and *Tremellini*, were successively described, and the characteristics of edible fungi were then pointed out. In conclusion, Mr. Howse gave a list of places in the district where the search for specimens could be successfully prosecuted.

The annual meeting of the club was held on Tuesday evening, March 21, Charles Stewart, Esq., M.R.C.S., F.L.S., President, in the chair.

The following gentlemen were elected as officers for the ensuing year:—President, Charles Stewart, M.R.C.S., F.L.S.; Vice-Presidents, W. T. Suffolk, F.R.M.S.; Robert Braithwaite, M.D., F.L.S.; and T. Rogers, F.L.S. Treasurer, B. Neighbour. Committee, N. Stowers, M.R.C.S., L.S.A.; J. W. Stephenson, F.R.A.S.; C. W. Stidstone; W. J. Parks; J. F. Wight; E. Dadswell; H. Helsham, M.D., F.R.C.S.; H. Robinson; and J. A. Smith. Honorary Secretary, F. Hovenden. Reporter, T. G. Ackland.

The Report of the Committee was then read by the Secretary. The committee congratulated the members on the continued increase in their number, the total of members on March 1 being 262. The Report then reviewed the work of the past year, and concluded as follows:

"The committee begin to feel that one of the objects of the club is taking root, namely, the gradual education of the members in microscopical science, and they venture to hope that soon the members, from their increased knowledge, will be able to enter more fully into the discussions, and also extend the number of papers given by them in the course of each year. In conclusion, the com-

mittee hope the coming year may be productive of increased activity and consequent prosperity."

The Report of the Committee having been unanimously adopted; the Treasurer's Report and Balance Sheet, in which a sum of 76*l.* 6*s.* 3*d.* was carried forward to the credit of the club, was then read, and it was unanimously agreed that it be received and adopted.

A vote of thanks to the officers of the club for their services during the past year was heartily accorded by the meeting.

The President then gave an address on "Eggs and Embryos," after which the meeting resolved itself into a conversazione.

An ordinary meeting was held on Tuesday evening, April 18, Charles Stewart, Esq., M.R.C.S., F.L.S., President, in the chair.

A paper was read by T. Charters White, Esq., M.R.C.S., on "The Aquarium as a Field of Microscopical Research." Mr. White gave a detailed account of the form of tank which he found most successful, and fully described the way in which it should be stocked; and he then gave an interesting account of some of the investigations which he had been able to make into the development of the animal and vegetable life which flourishes in an aquarium.

At the ordinary meeting held on Tuesday evening, May 16, Charles Stewart, Esq., M.R.C.S., F.L.S., President, in the chair, a paper was read by W. N. Hartley, Esq., F.C.S, on "Cavities in Crystals." As, however, the substance of this paper has already appeared in this Journal,* it will be unnecessary to reproduce it here.

At the above five meetings of the club, eighteen gentlemen, proposed as members, were balloted for and duly elected.

Adelaide Microscopical Club, South Australia.†

The twentieth meeting of the Adelaide Microscope Club was held on January 7, Mr. Gurner in the chair. Mr. Mais showed Möller's diatom plate with photographed names, under a Ross's binocular; also Lernean from shark. The Hon. Sec. brought nidamentary capsules of octopus. Mr. Gurner took for his subject, "Insect Structures from the Eucalyptus," showing insect eggs, and nests of most varied construction, found on the bark, leaves, and in the woody fibre of gum-trees.

The twenty-first meeting was held on February 4, Mr. C. W. Babbage in the chair. The subject chosen by the Chairman was "Cuticles of Leaves," specimens of native and other plants being shown in illustration. At the following meeting (March 3) the chair was taken by Dr. Gardner, subject "Urinary Deposits." Dr. Whittell showed section of an epithelial cancer from the tongue. Mr. Smeaton brought a very rich gathering of diatoms, and sponge and Holothuria spicula, recently obtained from Point Vincent, Biddulphias being particularly abundant. A number of interesting fishes

* Vol. xv. p. 170.
† Contributed by Thomas D. Smeaton, Hon. Sec.

from the same locality were also shown by Mr. Molineux. On the motion of the Rev. Jas. Jefferis, it was resolved to have a dredging excursion on the 18th current.

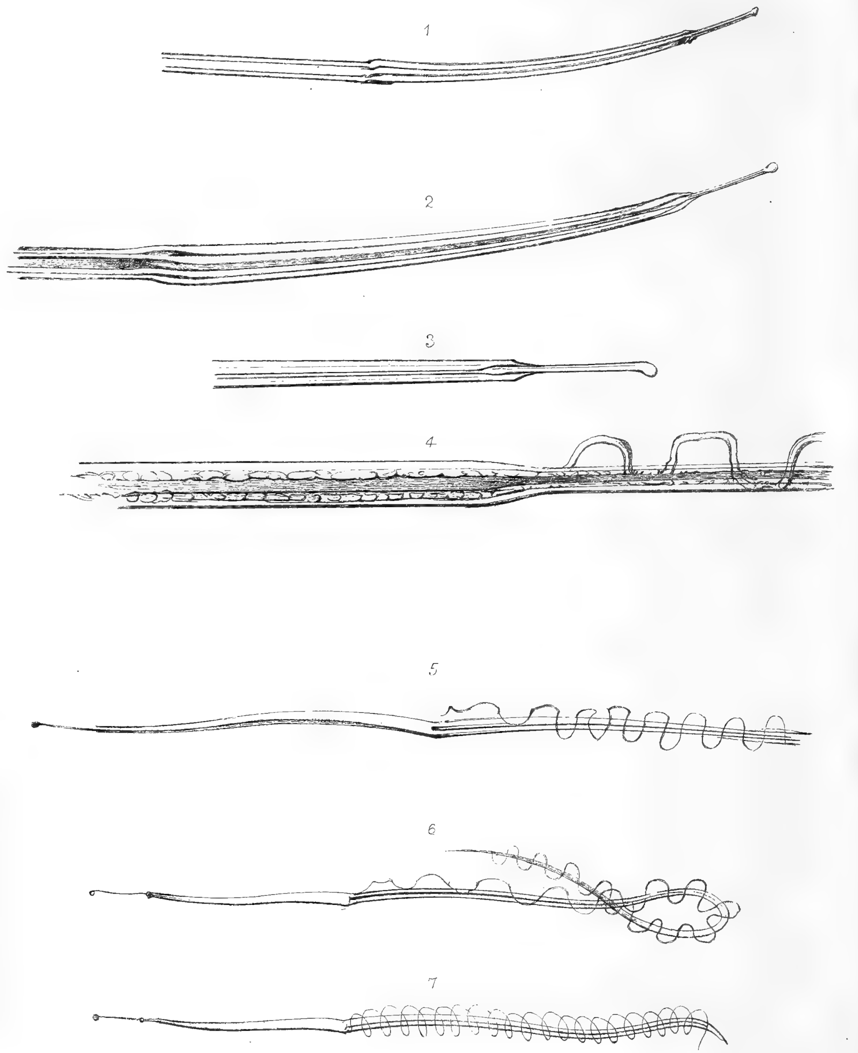
San Francisco Microscopical Society.

The regular meeting of the San Francisco Microscopical Society was held on Thursday evening, April 20, with a fair attendance of members.

Dr. Blake presented a slide, mounted with gold, which he had obtained from so-called telluride of gold, by treating with acid. The telluride was obtained from the American mine, Sunshine District, Colorado, and showed no gold before the application of acid, and Dr. Blake stated his belief that the pure gold was simply concealed by a superficial coat of tellurium.

Mr. Hanks exhibited two photographs of blood-corpuscles, each being from a slide with the blood of a dog and a cat, mounted in juxtaposition with that of a man, in order to show at a glance the relative size of the globules. The photographs showed a distinct difference between the disks of the cat's blood and that of man, but no appreciable difference in the case of the dog's blood.

Dr. Blake exhibited some blood obtained from a rabbit after the introduction of sulphate of thorium into the blood-vessels. Although the quantity used was only a grain, yet the physical properties of the blood had been very much altered, the blood-corpuscles having entirely lost their natural form, and presenting an indented outline, with numerous small highly refracting dots at the circumference. This marked influence on the blood-corpuscles tends to explain the highly poisonous character of the salt, death taking place in two minutes after the introduction of the above minute quantity into the circulation.



W. West & Co. sc.

Spermatozoa of Amphiuma Tridactylum.

THE

MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1876.

I.—*Observations upon Spermatozoa of Amphiuma tridactylum.*

By Christopher Johnston, M.D., Baltimore, U.S.A.

(*Taken as read before the* Royal Microscopical Society.)

Plate CXLV.

Towards the end of April in this year we received through the kindness of Professor J. W. Mallet, of the University of Virginia, two very fine Amphiumas from New Orleans. The larger one, measuring 3 feet in length, died on the journey; but the other, 30 inches long, survived, and on the 29th of April was made to exhibit the superb circulation of his giant blood-disks through the mesenteric vessels.

The next day the animal was found, upon dissection, to be a male; whereupon his organs were searched, and quantities of living moving spermatozoids met with. At first the size astonished us, notwithstanding our acquaintance with the average largeness of the histological elements of this other "friend of the physiologist;" then the general movements absorbed our interest; and lastly, the filmy modulations of a delicate appendage of the "body" fixed our attention. Masses of the elements resembled bundles of needles; but when separated, spontaneously or by art, they all glided along, either with an eel-like, coarse, flexuous motion, or with a gentle, arrowy progress. From the lower part of the efferent ducts the filaments obtained were completely rounded; but the spermatozoids met with at higher points did not completely unfold, for the "tail"

EXPLANATION OF PLATE CXLV.

1 to 4, dry preparations.

1. Head of spermatozoid.
2. Same, more highly magnified.
3. Apex and apicial filament.
4. Junction of head and body, with membrane beginning on the latter.

Living spermatozoids.

5. Head and projecting filament, body composed of two cords (and dorsal arch), and dorsal membrane.
6. Entire spermatozoid, immature, its tail being still looped.
7. Ideal, to show the effect of the membrane in motion.

Drawn from nature by the author.

was thrown forward so as to form a loop, in the area of which a most delicate membrane was seen to be adherent to the zoic filament. The end of the tail, however, was always free.

Several matters of interest were observed—the structure of the spermatic particles, the dimensions of their parts, and the length of the filaments.

The total length of each spermatozoon was $\frac{1}{85}$ of an inch. It begins at the moderately acute point of an elongated conical head $\frac{1}{312}$ of an inch in length by $\frac{1}{8128}$ of an inch in thickness; behind which the body, slightly more attenuate, ended almost into the vague. The "head" presented a small curve; but the body lay straight at times, or else threw itself into serpentine flexuosities.

A Tolles' recent $\frac{1}{10}$ immersion objective with that maker's $\frac{1}{2}$-inch ocular displayed many new points. The head, highly refractive and firmer than the other part, appeared to consist of a lamina of homogeneous substance bent along its axis, the two tumid edges of which met on the ventral side. Anteriorly these edges were suddenly bevelled away, and were lost by continuity in a delicate filament ending in a faint swelling.

Posteriorly the edges contracted at the junction with the body, but were seen to be continuous with two cords which had the same relation as their originals in the head. They lay close together, and from them sprang the dorsal arch, along the ridge of which the propelling membrane was attached.

This delicate film extended like a ruffle from the posterior extremity of the head as far as the caudal point; the main part was, however, invisible, but not so the margin, which was bordered with a thickened hem.

In a dry preparation the spermatozoids and their details were somewhat deformed, distorted, and spread out; still the characteristics of Amphiuma's seminal particles were very manifest when studied with a fine objective. Thus the edges of the cephalic folds and the somatic cords assumed a beaded appearance, as did also the collapsed arch-fold of the back along its margin. Yet the eye was not satisfied with the dried potential male elements, and reverted with continually renewed pleasure to the moving zooids.

But the charm of the whole study lay in the movements of these *things*, which advanced whether the tail swayed with a lashing motion or formed a straight line. From the junction of the head with the body to the extremity of the tail the double filament appeared encircled with the thread of a most delicate spiral, which seemed to wind from the head towards the caudal termination. And this direction was maintained so long as the zooid was free; but, what was indeed wonderful, the motion of the spiral was instantly *reversed* as soon as the head became attached or entangled, although the lashing or swaying motion continued if it had been executed

before. And again the downward winding was resumed if the detention was only momentary.

These reversals were frequently seen, and watched for some time.

When at rest the spiral resolved itself into a most delicate modulating membrane, of about the width of the body at its upper part, and extending from the slight constriction behind the spindle-shaped head all along the body and tail.

Of course we do not propose the dorsal membrane as a novelty, for that has been most thoroughly studied by Azermath and others; but attention is asked to the head, formed of a lamina bent into an elongated cone, and presenting a projecting apicial filament; to the twin cords of the body, apparently continuations of the tumid margins of the lamina, and supporting the dorsal arch; to the repeatedly observed reversal of the direction of the undulations of the dorsal membrane; and to the largeness of measure of the entire—creature, we had almost said—and of its several parts.

But these last are hardly to be wondered at in *Amphiuma tridactylum*, the giant red corpuscles of whose blood, immediately fresh from the living creature, we have found to attain to such dimensions as $\frac{1}{324}$ of an inch in their long axis, $\frac{1}{571}$ of an inch in their conjugate diameter, by $\frac{1}{3312}$ of an inch in thickness. These measurements were made with an eye-piece micrometer; but we find them to overrate the size of the corpuscles upon comparison with a series of eleven superb photographs just executed by our distinguished friend Col. J. J. Woodward, M.D., and this day received from him. These pictures, taken with a Tolles' $\frac{1}{10}$-inch objective, and a Tolles' $\frac{1}{8}$ inch, represent at once the disks and the micrometer, the divisions of which are rated at $\frac{1}{1000}$ of an inch. By careful computation we find the larger disks to range from $\frac{1}{383}$ of an inch in length to $\frac{1}{392}$ of the same measure.

Our fraction $\frac{1}{324}$ of an inch refers to the largest fresh corpuscles we met with, and must be compared with Col. Woodward's $\frac{1}{343}$ of an inch for the same reason; and if the latter be the true size of the greatest corpuscles, then our measurement of the spermatozoid of Amphiuma must suffer abatement in equal proportion.

For the photographs and for the permission to use them, we are happy to express our obligation to Col. Woodward.

Note.—It is but fair to add that my measurements of Amphiuma's blood were made upon instantly recent blood-disks; and that the photographs of Col. Woodward represented red corpuscles which had been preserved for thirty-six hours in a small vial deposited in a refrigerator, but which had a fresh appearance.

CHR. J.

II.—*On a New Form of Small Pocket Spectroscope.*

By H. C. Sorby, F.R.S., Pres. R.M.S.

(*Taken as read before the* Royal Microscopical Society.)

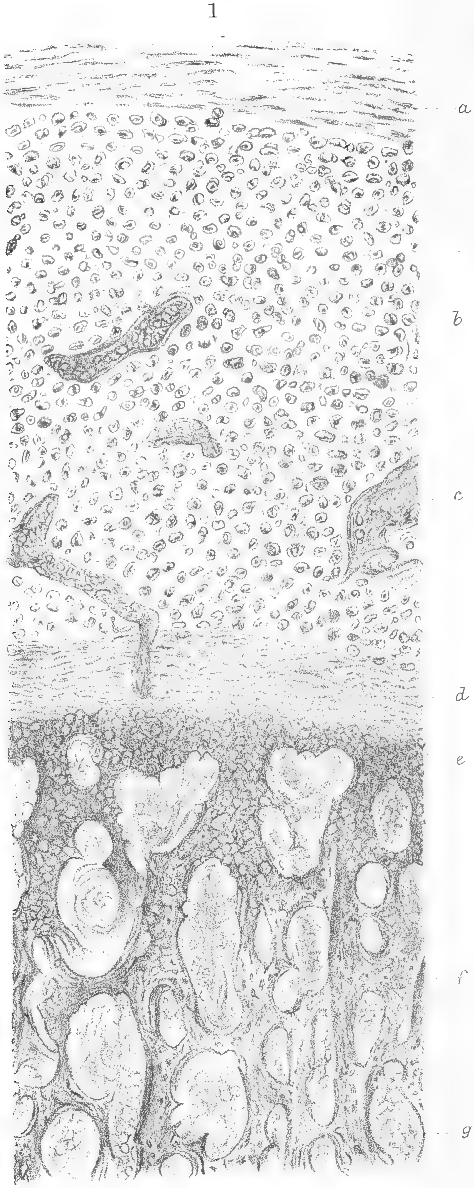
In carrying on inquiries with the spectrum microscope it is often very useful to have a small pocket spectroscope, to examine liquids in test-tubes or bottles, in circumstances that would make it very inconvenient to examine them with the microscope itself. By constantly carrying such a small spectroscope in my pocket I have also often been able to learn valuable facts that would otherwise have been overlooked. Medical practitioners might often at once decide whether a suspected solution contained blood, and could recognize small quantities of it, or of the degraded bile pigments, met with in urine in some diseases, by merely looking at it in a test-tube or bottle. But since one may carry about such an instrument for a long time without anything turning up that need be examined, it certainly ought not to be any larger than is compatible with efficiency. Such a small pocket spectroscope bears much the same relation to a complete spectrum microscope as a pocket lens does to an ordinary microscope, and therefore a description of a new small form of the instrument may, I think, be looked upon as sufficiently connected with the subjects treated of at the Royal Microscopical Society.

When examining Mr. Hilger's new contrivance for measuring the positions of any lines seen in a spectrum, as shown by him in the Loan Exhibition at South Kensington, he drew my attention to some small direct-vision prisms which gave a very considerable dispersion and had a relatively wide field, although their length was only about $\frac{3}{4}$ of an inch. He told me that he thought of making use of them for small pocket spectroscopes, and it occurred to me that it would be possible to mount such prisms in a somewhat unusual manner, which would have the advantage of making the instrument very small and compact.

The usual arrangement in such pocket spectroscopes is to mount the prisms between the eye and the lens used to bring the slit to focus. The focal length of this lens should not be less than $1\frac{1}{2}$ inch, and I find $1\frac{3}{4}$ a very convenient focal distance. If a higher magnifying power is used, the slit must be made inconveniently narrow to get good definition; the irregularities are greatly magnified, and the light too much reduced. A longer focal length is unnecessary, and undesirable for many reasons. Now with a focal length of $1\frac{3}{4}$ inch and the prism placed in the usual manner, the length of the instrument when in focus becomes about $2\frac{3}{4}$ inches; and taking into consideration the space necessary for the excentric

arrangement required for opening and shutting the slit, and for drawing and pushing one tube inside the other for focal adjustment, the total length of the instrument when closed up for putting into a case is at least 2¼ inches. There is thus length enough for a five-prism arrangement, and when that plan is adopted there is really no space lost. If such a length is thought to be no inconvenience, the ordinary arrangement is on the whole the best; but I would point out the very great advantage of having the lens made *achromatic*, since then all parts of the spectrum can be seen in tolerably good focus at the same time. However, when such an instrument is inside a brass box, the total length becomes 2½ inches, or even more, and ¾ inch in diameter. This becomes somewhat inconvenient when constantly carried in the waistcoat pocket. The arrangement which I proposed, and which has been carried out for me by Mr. Hilger, secures all the advantages of the larger instrument, and yet gives a length when closed up of less than 1½ inch, so that when in a small brass box the length is only a trifle more than 1½ inch, and the diameter ⅝ inch. The actual bulk is thus not one-half that of the old form, and is such that the instrument can without any inconvenience be carried along with other things in the waistcoat pocket, always at hand to examine anything that may accidentally present itself. Such small spectroscopes have indeed been made before, but the diminished size was obtained by sacrificing other advantages, and by making the focal length of the lens too small. The plan which I have adopted enables us to have the instrument very small, without sacrificing definition or brilliancy. This is accomplished by placing the compound direct-vision prism *between the slit and the lens,* so that the entire length of the instrument is that of the focal length, and not, as in the old arrangement, the length of the prism added to this focal distance. Perhaps this method of mounting a prism may appear to be very unusual and heterodox, but it does really give a very satisfactory result. The prisms consist of a single very dense flint prism of 106°, and two crown of 98°. This combination gives quite as great a dispersion as is desirable for the purposes to which such an instrument is likely to be applied. The eye lens is made achromatic, and of 1¾ inch focal length, which is increased to 2 inches by looking through the prisms, for which it is of course properly corrected. It then shows the principal Fraunhofer lines quite distinctly, A, D, F, and G being visible at the same time, but the effect of looking through the prism is to make the focal length greater in one direction than in another. In order to bring the two sides of the spectrum into focus along with the spectrum itself, it is therefore necessary to insert below the prisms a plano-cylindrical lens of 2¼-inch focus, with its axis in the line of the axis of the prisms. This makes the focus 1¾ inch long in every direction;

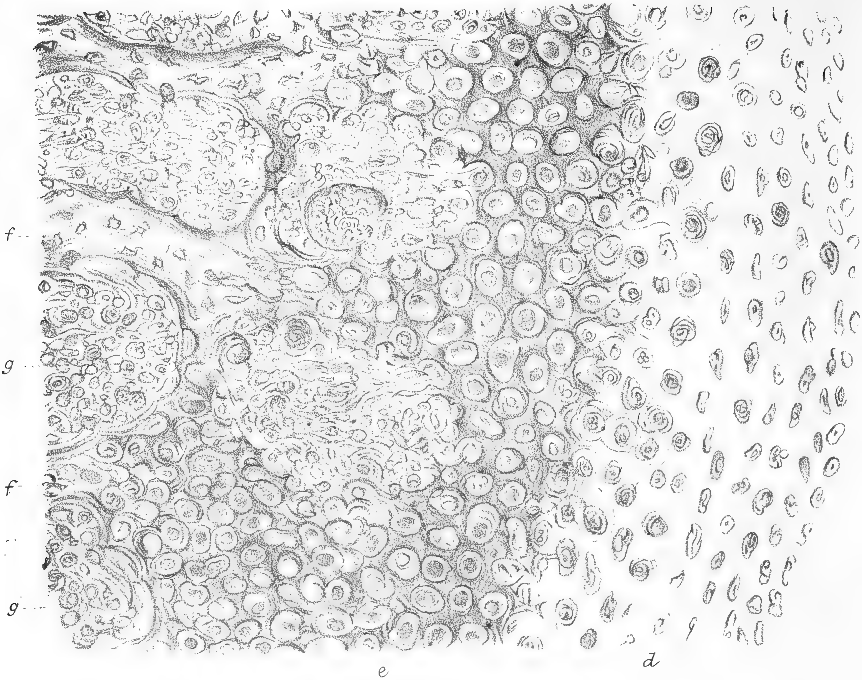
and, acting as a field lens, also increases the amount of light. The result is that, although the dimensions of the instrument are so small, the dispersion, definition, and focal length for all parts of the spectrum are in every respect satisfactory for the examination of the class of objects for which it is designed. At the same time I must say that I should not adopt this construction when an increase in the size of the instrument is of no importance. The dispersion is diminished, and the definition is certainly not improved; and we must be content if it is as good with an instrument half the size, since that makes all the difference between what is convenient and inconvenient for constant carriage in the pocket. In conclusion, I would say that I do not think it desirable to fix a reflecting prism over part of the slit of such spectroscopes, for the purpose of comparing two spectra. This can never be done in a satisfactory manner with such an instrument, and the prism makes it longer, and prevents small objects from being brought sufficiently near to the slit. It should be looked upon as a very handy spectroscope, and not as a spectrometer.



W. West & Co. lith.

Ossification Process in Birds.

2



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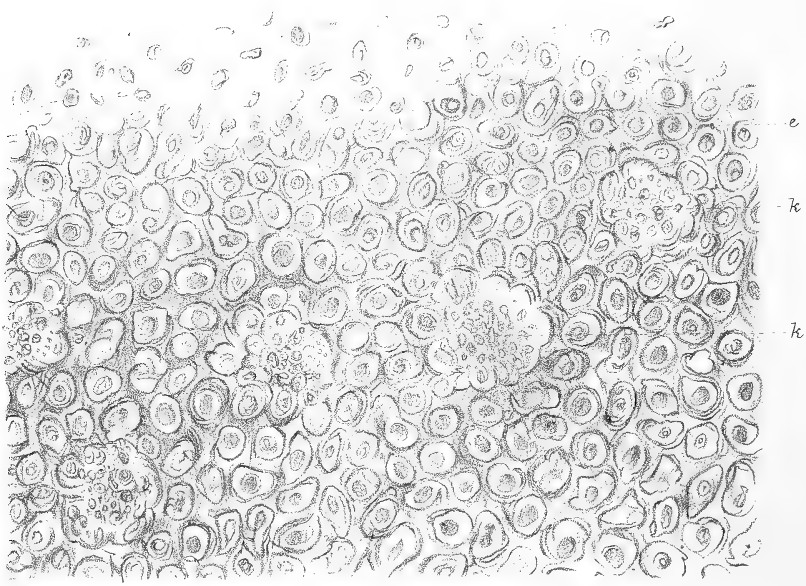

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Ossification Process in Birds.

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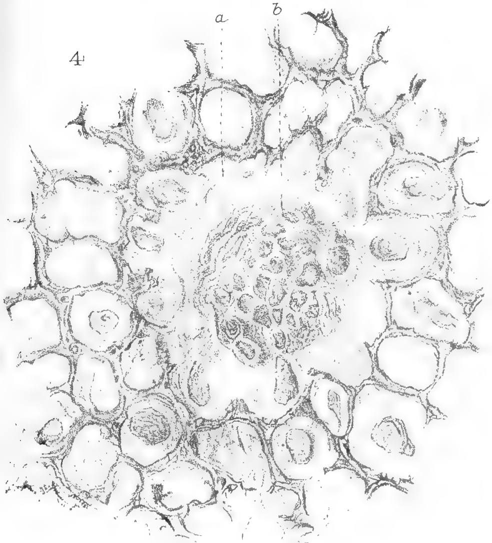

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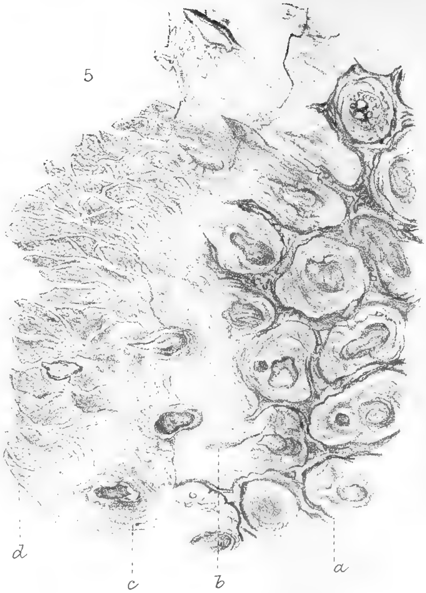

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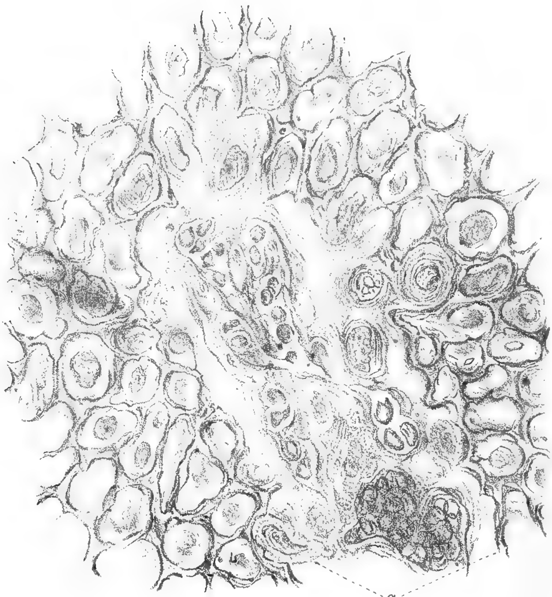

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Ossification Process in Birds.

III.—*On the Ossification Process in Birds, and the New Formation of Red Blood-corpuscles during the Ossification Process.**

By Dr. L. Schöney.

Plates CXLVI., CXLVII., and CXLVIII.

Contrary to the assertion of Virchow, Heinrich Müller, and other more recent investigators, that marrow tissue proceeds direct from cartilage, other views have recently been published, according to which the elements of the marrow are said to have immigrated either from the blood-vessels or the periosteum. In the same manner remained the question up to this day debatable, in which way the bones originate, notwithstanding the pioneering researches of Heinrich Müller. I thought it therefore desirable to study this question on such objects which, as far as I know, have not been yet examined, viz. cartilage and bone of *birds*.

The following statements refer to a number of fowl and pigeons of various ages. Immediately after death of the animals, I put the exarticulated legs, which were quickly freed from the muscles, into some yellow solution of chromic acid, to which I added a few drops of muriatic acid. A few days afterwards I prepared

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EXPLANATION OF PLATES CXLVI., CXLVII., AND CXLVIII.

Fig. 1.—Vertical section through the joint cartilage of a young fowl × 250. *a.* Zone of flat spindle-shaped cartilage corpuscles immediately at the knee-joint surface. *b.* Zone of the round nuclei-holding cartilage corpuscles, with blood-vessel enclosing marrow-spaces *c.* *d.* Yellowish-red zone of small thickly-crowded flat cartilage corpuscles. *e.* Zone of the lime-deposited elementary substance of the cartilage, up to which reach the ready-formed bone spicules *f.* Between these are the first-formed marrow-spaces of the bone *g.*

Fig. 2.—Vertical section of the same × 450, marked as Fig. 1. In the marrow-spaces of the bone variously shaped marrow elements, and between these multi-nuclear protoplasm bodies.

Fig. 3.—Horizontal section through the knee-joint cartilage of a young fowl, close to the ossification border × 450. *e.* Lime structure of the cartilage, which appears interrupted by interspersed solution spaces of the cartilage elementary substance *k k.*

Fig. 4.—Portion of the same preparation × 700. To the cartilage, which contains a certain amount of lime structure, is attached a zone *a*, in which the lime salts are dissolved. Rigidly bordered by this zone is visible a new-formed marrow-space *b.* In the protoplasm of the latter are interspersed homogeneous lustrous small particles of various forms.

Fig. 5.—Segment of a little bone spicule engaged in formation × 700. *a.* Cartilage with grate-like lime structure. *b.* Cartilage with hyaline elementary substance. *c.* Zone of the new-formed earliest bone, with interspersed star-like bone-corpuscles. *d.* Zone of the osteoblasts, engaged in transition into true bony substance.

Fig. 6.—Oblique section through the ossification border of the bone of a young fowl × 700. The marrow-space *a* is surrounded by hyaline cartilage with lime structure. In the marrow-space are visible various nuclei with pointed ends, in the interior of which lie homogeneous lustrous small particles, hämatoblastæ. In the lowest vascular space the red blood-corpuscles are still without nuclei.

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* Translated from Schultze's 'Archiv' by W. R.

vertical and horizontal sections of cartilage and bone, principally of the knee-joint. A number of these sections were cleared with glycerine, and therein preserved (a method which I prefer to alcoholic or turpentine treatment, and preservation in balsam, because the clearing in the latter case will soon reach such a degree as effectually to interfere with the study of the more delicate tissues).

My observations and drawings have been made with Perick's objectives 5, 9, and 10 (immersion); the drawings are not made diagramatically, but with the utmost possible fidelity direct from the preparation. For greater clearness I shall state my observations in two sections; the first treating of the ossification, the second of the new formation of red blood-corpuscles.

I. *The Ossification Process.*

Heinrich Müller,* as we know, was the first who denied the direct transformation of cartilage into bone tissue; but who would have instead marrow formation precede the bone formation. C. Heitzmann † especially (this view having since repeatedly been confirmed) maintained the transformation of cartilage tissue into marrow tissue, this having been preceded by the deposit of lime, and the solution of the elementary substance; and besides developed a theory of bone formation which most closely coincided with his view of the structure of cartilage and bone.

He proved the existence of a rich net of living matter in the interior of the elementary substance of the hyaline cartilage, and showed that all the cartilage corpuscles (cartilage cells) are united in one uninterrupted mass. According to him, the solution of the elementary substance would be the only thing required to render chondrin-infiltrated-protoplasm again visible, by which the same would be divided into new elements, known as "marrow-cells." These elements themselves, however, are directly connected with one another by living matter, even where there has been formed an apparently structureless soft elementary substance (mucine ?).

Vertical sections through the knee-joints of birds of various ages show, in the first place, in accordance with C. Heitzmann's observations on dogs, cats, and rabbits, that with increasing age the cartilage mass decreases in circumference (volume); next, that the transformation of the cartilage directly into marrow elements, and indirectly into bone, takes place only in young animals, whilst in older ones the ready-made bone directly joins the cartilage, and in fully-grown pigeons the marrow-spaces of the bone form prolongations which extend into the cartilage to a certain height.

* 'Zeitschrift für wissensh. Zoologie,' 7 Band.
† 'Wiener med. Fahrb.' 1872.

Studying the vertical sections of young animals, under slight magnification, the following will be seen:

Fig. 1. On the free part of the cartilage which forms the front of the knee-joint, also near the perichondrium, are seen spindle-shaped and elongated cartilage corpuscles (*a*), by degrees changing into round cartilage cells, having distinct nuclei. The latter form a large stratum (*b*) which appears to be intersected by vascular cartilage, which forms marrow-spaces (*c*) of the most various forms. Joined to the stratum of round nucleated cartilage corpuscles is a narrow layer, formed of small flat cartilage bodies (*d*), which is invariably coloured yellowish red, and immediately joins the true bony border. The bone formation of the elementary substance (*e*) produces a handsome structure, going over into the first-formed small beams of the bone tissue (*f*). The latter border the marrow-spaces (*g*) of the bony epiphyses (Epiphysenknockens).

Under greater enlargement (Fig. 2) one observes that the lime structure (*e*) reaches, in its uppermost little pointed processes, to the layer (*d*). In the interior of the lime structure the cartilage corpuscles are distinctly visible. In different spots the small bone beams reach directly up to the lime structure, without any sharp defined border being recognizable between both; the bone, however, being sufficiently distinguished by the layer of bone-corpuscles in it. The marrow-spaces (*g*) are filled with round, oblong, or spindle-shaped marrow elements, besides which there are often found various large, evenly granulated protoplasma layers, either with many nucleated or non-nucleated so-called *Myeloplaxes* (Robin). Spindle-shaped elements are most frequently found in the centre of a marrow-space, where they are connected with blood-vessels, as I shall show hereafter.

The marrow formation can best be followed in horizontal sections (Fig. 3). First we observe that in certain spaces a secretion of the cartilage elementary substance (*e*) takes place. The immediate transformation of the cartilage into free protoplasm with formation of lustrous small lumps (*k k*) which fill the marrow-spaces, cannot, it is true, be directly observed; however, we are bound to conclude that it does take place, because the marrow-spaces are surrounded on all sides by cartilage tissue, and the lime structure broken up, as it were, reaches into the marrow-spaces. It can be observed that first of all a cartilage-tissue unit (territorium) appears deprived of all its lime salts, and next to it, apparently through direct transformation of the cartilage corpuscles, emerge marrow elements, whilst immigration from the periosteum at such a distance cannot be thought of. Just as little is an immigration from the blood-vessels to be supposed, such as, amongst others, A. L. Rollet took for granted, because in the newly-formed marrow-spaces there are no blood-vessels to begin

with; and secondly, these, when they are first formed, have as yet no connection with the old blood-vessels.

The transformation of cartilage into marrow elements is visible in Fig. 4. The limeless zone of the cartilage (*a*) is suddenly replaced by a protoplasm, in which are interspersed numerous lustrous small particles. The latter generally appear surrounded by a small bright margin, through which, with careful illumination, are seen small trabeculæ, which, on account of their lustre and homogeneous structure, remind us of one of the formations in mammals described as "hämatoblasts. Sections such as those rendered in the drawing, force us so to say to the supposition that not only the cartilage corpuscles, but the whole living matter, which is gathered in the interior of the cartilage elementary substance, participate in the marrow formation. The sudden transition from apparently structureless elementary substance to a free protoplasm layer, without the least indication of division of the cartilage corpuscles, would scarcely admit of any other explanation.

Frequently there are found in the solution border of the elementary cartilage substance, cartilage corpuscles, which partly are placed in the same, and partly already reach into the first-formed marrow-space, and never have I observed in such corpuscles any division whatever. If people, notwithstanding the idea of division of the "cartilage cells," adduce as proof two close-together-lying elements, or elements with two or more nuclei, they simply forget that the hard elementary substance does not admit of enlargement of the elements, which surely ought to precede the simple division. The latter will certainly have first to be secreted before the protoplasm can show signs of life to a full extent; and to me the supposition appears to be more in accordance with the fact of the matter, that a new formation of living material takes place in certain centres of the just freed protoplasma (even thus conducing to the formation of the compact lustrous small particles), than the clinging to the diagramatic "cells-partition theory," which certainly no facts justify. The so frequently visible multinuclear protoplasm bodies (Myeloplaxa) will according to this view have simply to be considered as freed cartilage-tissue units (territories), and we very easily understand that sometimes a number of such units are secreted to a large common protoplasm layer before further changes take place in the latter.

So much appears to me to be out of the question, that in birds, just as in mammals, cartilage tissue becomes direct marrow tissue. The next question then will be: How is bone tissue formed out of marrow tissue? This question too has had many different answers. The progress in our knowledge of the histogenesis, however, is not less visible here than in other tissue-fields.

Gegenbaur was the first who considered the "osteoblast" as

offspring of the marrow elements, and as the real bone-forming element. He still adhered, it is true, to the secretion theory, because it might be considered the most simple one. Herr Waldeyer pronounced for the direct transformation of one part of the osteoblast into elementary bone structure in Max Schultze's sense; and lastly, C. Heitzmann observed that during the transformation of the osteoblasta in bony elementary substance, the living matter in the interior of the same was preserved, whilst the protoplasm liquid only, changed into a gelatin-giving one.

Fig. 5 illustrates the process with an almost diagramatic clearness. This drawing is taken at the spot where direct new-formed bone joins the limeless cartilage. We see limeless cartilage (*a*), then a hyaline zone (*b*). This zone joins the bone elementary substance (*c*) with a sprinkling of the characteristic bone-corpuscles; and lastly, we see a layer of osteoblasts (*d*). On the border between C and D are seen all the transitions of free (not infiltrated) osteoblasts into those which disappear in elementary substance. The tender granulation of the newly-formed osteogenous substance, however, indicates that the structure of the osteoblasts did at least not entirely disappear during the formation process of the bone.

I can therefore confirm the view that the flat protoplasm bodies, which Gegenbaur called osteoblasts, are real bone-formers; I also have seen that the osteoblasts are connected among one another by delicate threads (Gegenbaur's thorns). Equally must I maintain the transformation of one part of the osteoblasts into bone elementary substance in Waldeyer's meaning. But I am not prepared to assert that in the bones of birds the osteogenous elementary substance contains living matter; I only am bound to observe that the bone of birds is constructed exactly as in mammals, as I have been taught, by longitudinal and transverse sections through femur and tibia.

Here too we find lamina systems arranged round the vascular canals; here too we find in the cavities of the elementary substance protoplasmic bodies which are described as bone-cells; here too we find the well-known star-like projections. But how far the bone, as regards contents of living matter, accords with that of the mammals, future researches alone will show.

II. *New Formation of Blood and Blood-vessels.*

I have studied the new formation of blood with special care. E. Neumann* has denied the theory of the blood formation as regards the new formation of red blood-corpuscles on the ossification border of the cartilage. At the same time he drew attention to

* Heitzmann's "Hämatoblasten," 'Archiv für mikros. Anatomie,' Nov. 1874.

Aeby as the inquirer who first maintained this blood formation. Aeby, however, only conjectured this blood formation on the ossification border in the cited work of his, "On the Symphisis Pubes of Man, with contributions to the study of Hyaline Cartilage, and its Ossification," * as I shall show.

On page 54, i. c., he says: "The quantity of blood-corpuscles is surprising, which is found sometimes in the marrow-spaces, even long before the formation of vessels, and if it is possible, nay probable, that the same have entered from elsewhere, the circumstance, however, that next to the cells there are often found most numerous nuclei-like formations, entirely analogous in size and form to the blood-globules, and the most gradual transitions of which, specially as regards the flattening, unto the ready-formed blood-corpuscles, are met with, justifies the inquiry whether or not there exists between both a genetic connection. My own researches in this direction have not led to any result."

From this it follows that Aeby guessed the new formation of red blood-corpuscles on the ossification border, and expressly declares that his investigations, as regards the new formation of blood, led to no result; whilst Heitzmann maintained this new formation as a fact.

I cannot recognize the validity of the arguments which E. Neumann urges against Heitzmann's exposition; for instance, he finds it incomprehensible that the hämatoblasts are coloured by carmine, whilst the ready-formed blood-corpuscles, as we know, are not.

But Heitzmann expressly says that the hämatoblasts, homogeneous, yellowish, lustrous, small particles, represent the infancy stage of the protoplasm, and that from them, afterwards and after undergoing certain changes, proceed real red blood-corpuscles. Bodies in the young condition no doubt have different reactions to colours to what they have when old.

E. Neumann further misses the nuclei in the hämatoblasts; and because he adopts the usual opinion that the blood-corpuscles in their infancy do possess nuclei—afterwards, however, none—he is unable to look upon the hämatoblasts as red blood-corpuscles in their stage of infancy.

My researches are specially adapted to clear up this point.

El. Metschnikow † found in the embryo fowl first blood-corpuscles little coloured and having nuclei, and afterwards distinctly-coloured but non-nucleated blood-corpuscles; and concluded therefrom that the latter originated in the former. This conclusion is not quite justified, because from one and the same source blood-corpuscles may originate, which at first are less and afterwards

* 'Zeitschrift für rat. Med.' 3te Reihe, 4ter Bd. 1858.

† Virchow's 'Archiv,' 41 Bd. 1867, "Zur Entwickelungs-geschichte der rothen Blutkörperchen."

more coloured, and get in circulation without a transition from one to the other being absolutely necessary. The same objection may also be applied to the blood-corpuscles of the mammals. If there are in the beginning visible nucleated, and afterwards non-nucleated blood-corpuscles, who can prove that the latter originated in the former, and that every red blood-corpuscle has passed through a stage in which it possessed a nucleus? *

Let us contemplate Fig. 6, which represents an oblique section of the ossification border of the limb of a growing fowl. We observe in the centre of a secretion space (Aus-schmelzungsraumes) (*a*) homogeneous lustrous small particles, lying in irregular spaces which are bounded by spindle-shaped elements, and of which one can convince oneself with the aid of the adjusting screw. One is further convinced that the little knobs become pointed at the ends at least in one direction, and are in no wise connected with already formed blood-vessels. These little particles are by writers unanimously declared to be the first vessel formations, which, in mammals as well, appear in the centre of each marrow-space, where the blood-vessels also gradually disappear in ready-formed marrow-spaces. There is no doubt that the small spindles in the section belong to vascular endothelia; but what are the lustrous corpuscles in the centre of the marrow-spaces?

Bodies lying in the surroundings of unfinished or ready-made blood-vessels may indeed be described as blood-corpuscles; the corpuscles, however, which are before us, are no blood-corpuscles, but hämatoblasts only to begin with. They are distinguished, as mentioned before, by a homogeneous structure, and a peculiar yellowish-greenish lustre. I cannot attribute any very great importance to the colour itself, because the parts which have lime deposited in them too have a yellowish-greenish lustre, whilst the ready-made blood-corpuscles in my preparations are often wanting in this colour. Surprising, however, is it that there are no nuclei visible in these corpuscles, and still more so that the blood-vessels which lie lower down towards the ready-formed bone, and the connection of which with older blood-vessels cannot be proved with certainty, are brimful with blood-corpuscles which show no nuclei. Only in lower vascular formations appear the characteristic, oblong nucleated corpuscles of the bird's blood. If it must now be admitted that the latest marrow formations must be looked for close to the ossification border of the cartilage, the older ones, however lower down, and if further in so small a space as, relatively speaking, the field of some medullary spaces is, are found such manifold formations, which must be pronounced as blood-corpuscles, then the idea that the nucleus is by no means an early, but, on the contrary, a rather late formation in the blood-corpuscle (as besides

* We think this is clearly proved by reference to the blood of the *Camelidæ*.—Ed. 'M. M. J.'

E. Brücke already has said of other protoplasm bodies), must become a recognized one.

I cannot explain the representations, of which one is given in Fig. 6, in any other way than by saying thus: The first-formed blood-corpuscles, the hämatoblasts, are without nuclei, and the ready-formed blood-corpuscles alone have any. We cannot search for the sources of the blood formation in mammals without regarding the nucleus as a postulate (ein postulat) of the stage of youth.

Whilst I regard as certain the new formation of red blood-corpuscles out of the hämatoblasts on the ossification border of the hyaline cartilage in growing birds, I must, on the other hand, observe that this new formation does not take place in full-grown animals.

Even in a nine-months' old pigeon, ready-formed bone tissue, with flat tissue containing medullary spaces, joins immediately a layer of round cartilage corpuscles, which show no cartilage marrow-spaces. The blood-vessels of these marrow-spaces form loops at the upper (towards the cartilage turned) ends. The intermediate stage of the lime deposit of the elementary substance, as well as any representations which might signify a new formation of blood-vessels and hämatoblasts, are wanting here. In still older animals, where the layer of hyaline cartilage appeared still more reduced, the upper ends of the marrow-spaces were isolated from the cartilage by means of lamellated layers of bone tissue.—*Max Schultze's Archiv*, Band xii.

IV.—*On a Possible Explanation of the Method employed by Nobert in Ruling his Test-Plates.* By William A. Rogers.

I recognize the fact that no explanation of a purely mechanical process can be regarded as either satisfactory or final, which does not answer the crucial test of reproduction. I offer to the reader what I believe may prove to be an explanation of the process followed by Nobert in ruling his test-plates; the highest band which has been resolved under the microscope, reaching 112,600 lines to the inch. You properly ask me if I can reproduce these rulings. I frankly answer that I cannot. Indeed, I can hardly hope ever to succeed in producing lines which combine the wonderful delicacy, uniformity, and distinctness found in nearly all of Nobert's plates. But I have reached what I hope may prove to be a useful approximation to Nobert's results. Beginning with 2000 lines to the inch in 1871, I have now little difficulty in reaching 60,000, the width of each line being a little less than one-half of the intervening space. Several of my plates have been correctly counted as

far as 80,000 to the inch; the observer having no knowledge of the actual number ruled. Two plates in the possession of Frederick Habirshaw, Esq., of New York, contain bands proceeding by 10,000 as far as 120,000 to the inch. The bands of both these plates were correctly counted by Samuel Wells, Esq., of Boston, as far as 80,000, but beyond that point the number counted was less than the number ruled. While the lines of the higher bands seem to be nearly as distinct as Nobert's, they are by no means as smooth and uniform throughout their whole length.

The theory which I offer to the Academy is wholly the outgrowth of my own experience. In the various experiments which I have made, I have noted the constant recurrence of certain results under certain conditions, and these results form the basis of my conclusions. Whether they form a true explanation of Nobert's process is, of course, entirely a matter of conjecture. I am well aware of the risk incurred in offering a theory which can at once be refuted by a single stroke of the pen. Nobert has well kept the secret of his process. If I have failed to detect it, it is easy for him to say, "You are wholly mistaken." Even if this proves to be the case, the facts developed in the course of my experiments may possess sufficient interest to warrant their publication.

The problem is naturally divided into two parts:

(*a*) The mechanical operation of moving the plate to be ruled over given and equal spaces.

(*b*) The operation of producing on glass, lines of varying degrees of fineness.

If a screw is employed to give the required motion, it would seem at first sight very easy to reach any desired limit of accuracy. In my own machine, the head of the screw, which is 11 inches in diameter, is divided into 100 equal parts. For subdivisions, a microscope is employed, having an eye-piece micrometer, 100 divisions of which exactly cover one division of the screw-head. It is therefore easy to read directly to $\frac{1}{10000}$, and by estimation to $\frac{1}{40000}$ of a revolution. Since the pitch of the screw is $\frac{1}{24}$ of an inch, these numbers correspond to a motion of $\frac{1}{240000}$ and $\frac{1}{960000}$ of an inch. By a device which I shall presently describe, the subdivisions can be carried to $\frac{1}{100000}$ of a revolution.

But nothing can be farther from the truth than to suppose that, because this high limit of theoretical accuracy can be reached, therefore the lines ruled are separated by spaces accurate within the same limits. It is difficult to name the lowest limit of deviation from the truth which it is possible to reach; but I have long since despaired of being able to rule, e. g. 100 lines, covering successive revolutions of the screw which shall contain no errors of any kind, whether individual or accumulated, greater than $\frac{1}{30000}$ inch. I have availed myself of every opportunity to measure the ordinary

stage micrometers furnished by dealers in microscopes, and I find the usual range of error to be between $\frac{1}{2000}$ and $\frac{1}{10000}$ of an inch.

Of course the average error may fall far within these limits; and, especially when the lines are closely ruled, the individual errors may seem by comparison insignificant; but I have been unable to find *any* rulings which invariably surpass the limit which I have named. As an illustration of the limit of accuracy attainable, I give in Tables I., II., and III., measurements of an excellent Nobert diffraction plate, a Rutherford diffraction plate, and a plate ruled by myself for the purpose of investigating the errors of my screw. The measures were all made with a $\frac{1}{15}$th objective, and an eye-piece micrometer, 200 to the inch, the lines of which were about $\frac{1}{18000}$ of an inch in breadth. Using a B ocular, the value of one division was found to be $\frac{1}{15200}$ of an inch. With this arrangement, it was found easy to measure any given space to $\frac{1}{50000}$ of an inch with considerable certainty. To eliminate errors in the micrometer, the same divisions were used in all comparisons. In the Nobert plate, the width of the lines is about $\frac{1}{18000}$ of an inch; in the Rutherford plate, $\frac{1}{35000}$ of an inch; and in my own, $\frac{1}{28000}$ of an inch. The space measured was $\frac{1}{240}$ of an inch.

Table I. contains the residuals obtained by subtracting the measure of each space from the mean of all the spaces.

Table II. contains the residuals obtained by subtracting the error of each space from the error of the next consecutive space.

Table III. contains the periodic errors deduced from Table I.

It is quite evident that in the three cases under consideration, there are numerous accidental errors amounting to $\frac{1}{30000}$ of an inch and more, while in the last case the evidence of periodicity is very decided; its value at the maximum point being $\frac{1}{5600}$ of an inch. An examination of the values in Tables I. and II., column III., will show how easy it is to be misled by a seeming accuracy when only consecutive spaces are measured. It is only when the errors become magnified by successive increments that they attract attention.

The following will be found a very convenient and accurate method for measuring directly the magnitude of the periodic errors.

First, a series of equidistant lines is ruled on thick glass, care being taken to use glass having a plane surface. It is better also to have the spaces correspond to equal parts of a revolution of the screw. On one side a heavy finding line is ruled. This band is then reproduced on microscopic cover-glass, having a thickness of about $\frac{1}{100}$ of an inch. Of course care is taken to use the same part of the screw, and the same divisions of a revolution as before. By cementing the glasses together with balsam, face to face, but with the finding lines on opposite sides, the periodic errors, if they exist, will appear under the microscope with twice their real magnitude.

TABLE I.

RESIDUALS FROM THE MEAN.

Spaces.	I. Nobert.	II. Rutherford.	III. Rogers.
	fraction of inch.	fraction of inch.	fraction of inch.
1	$+\frac{1}{21700}$	$-\frac{1}{33800}$	$-\frac{1}{44700}$
2	$-\frac{1}{76000}$	$+\frac{1}{27600}$	$-\frac{1}{44700}$
3	$+\frac{1}{38000}$	$-\frac{1}{30700}$	$-\frac{1}{23800}$
4	$-\frac{1}{76000}$	$-\frac{1}{101000}$	$-\frac{1}{34500}$
5	0	$-\frac{1}{27600}$	$-\frac{1}{34500}$
6	$+\frac{1}{38000}$	$-\frac{1}{43400}$	$-\frac{1}{44700}$
7	$+\frac{1}{16900}$	$-\frac{1}{33800}$	$-\frac{1}{108600}$
8	$-\frac{1}{21700}$	$-\frac{1}{101000}$	$-\frac{1}{380000}$
9	$-\frac{1}{152000}$	$+\frac{1}{307000}$	$+\frac{1}{253000}$
10	$-\frac{1}{38000}$	$+\frac{1}{101000}$	$+\frac{1}{27200}$
11	$-\frac{1}{50700}$	$+\frac{1}{33800}$	$+\frac{1}{27200}$
12	$+\frac{1}{76000}$	$-\frac{1}{33800}$	$+\frac{1}{20000}$
13	$+\frac{1}{152000}$	$+\frac{1}{307000}$	$+\frac{1}{33100}$
14	$-\frac{1}{76000}$	$-\frac{1}{101000}$	$+\frac{1}{42200}$
15	$+\frac{1}{19000}$	$+\frac{1}{43400}$	$+\frac{1}{57500}$
16	$+\frac{1}{50700}$	$-\frac{1}{60800}$	$+\frac{1}{23100}$
17	$-\frac{1}{19000}$	$-\frac{1}{307000}$	$-\frac{1}{108600}$
18	$-\frac{1}{30400}$	$-\frac{1}{307000}$	$-\frac{1}{380000}$
19	$-\frac{1}{50700}$	$+\frac{1}{43400}$	$-\frac{1}{34500}$
20	$+\frac{1}{152000}$	$+\frac{1}{43400}$	$-\frac{1}{44700}$

TABLE II.

CONTIGUOUS ERRORS.

Spaces.	I. Nobert.	II. Rutherford.	III. Rogers.
	fraction of inch.	fraction of inch.	fraction of inch.
1	$\frac{1}{16900}$	$\frac{1}{15200}$	$\frac{1}{50700}$
2	$\frac{1}{25300}$	$\frac{1}{25300}$	$\frac{1}{76000}$
3	$\frac{1}{25300}$	$\frac{1}{152000}$	0
4	$\frac{1}{76000}$	$\frac{1}{21700}$	$\frac{1}{152000}$
5	$\frac{1}{38000}$	$\frac{1}{16900}$	$\frac{1}{76000}$
6	0	$\frac{1}{152000}$	$\frac{1}{152000}$
7	$\frac{1}{30400}$	$\frac{1}{50700}$	$\frac{1}{152000}$
8	$\frac{1}{9500}$	$\frac{1}{76000}$	$\frac{1}{30400}$
9	$\frac{1}{25300}$	$\frac{1}{152000}$	0
10	$\frac{1}{50700}$	$\frac{1}{50700}$	$\frac{1}{76000}$
11	$\frac{1}{152000}$	$\frac{1}{16900}$	$\frac{1}{50700}$
12	$\frac{1}{30400}$	$\frac{1}{30400}$	$\frac{1}{152000}$
13	$\frac{1}{15200}$	$\frac{1}{76000}$	$\frac{1}{152000}$
14	$\frac{1}{50700}$	$\frac{1}{30400}$	$\frac{1}{38000}$
15	$\frac{1}{15200}$	$\frac{1}{25300}$	$\frac{1}{19000}$
16	$\frac{1}{30400}$	$\frac{1}{50700}$	$\frac{1}{152000}$
17	$\frac{1}{13800}$	0	$\frac{1}{38000}$
18	$\frac{1}{50700}$	$\frac{1}{76000}$	$\frac{1}{152000}$
19	$\frac{1}{76000}$	0	0

TABLE III.

PERIODIC ERRORS.

Spaces.	I. Nobert.	II. Rutherford.	III. Rogers.
	fraction of inch.	fraction of inch.	fraction of inch.
1	$+\frac{1}{21700}$	$-\frac{1}{33800}$	$-\frac{1}{44700}$
2	$+\frac{1}{30400}$	$+\frac{1}{152000}$	$-\frac{1}{22400}$
3	$+\frac{1}{16900}$	$+\frac{1}{30400}$	$-\frac{1}{11500}$
4	$+\frac{1}{21700}$	$-\frac{1}{152000}$	$-\frac{1}{8700}$
5	$+\frac{1}{21700}$	$+\frac{1}{33800}$	$-\frac{1}{6900}$
6	$+\frac{1}{13800}$	$+\frac{1}{152000}$	$-\frac{1}{6000}$
7	$+\frac{1}{7600}$	$-\frac{1}{43400}$	$-\frac{1}{5700}$
8	$+\frac{1}{11500}$	$-\frac{1}{30400}$	$-\frac{1}{5600}$
9	$+\frac{1}{12700}$	$-\frac{1}{33800}$	$-\frac{1}{5700}$
10	$+\frac{1}{19000}$	$-\frac{1}{50700}$	$-\frac{1}{7200}$
11	$+\frac{1}{30400}$	$+\frac{1}{100300}$	$-\frac{1}{9900}$
12	$+\frac{1}{21700}$	$-\frac{1}{50700}$	$-\frac{1}{19500}$
13	$+\frac{1}{19000}$	$-\frac{1}{60800}$	$-\frac{1}{47000}$
14	$+\frac{1}{25300}$	$-\frac{1}{38000}$	$+\frac{1}{380000}$
15	$+\frac{1}{10900}$	$-\frac{1}{30400}$	$+\frac{1}{50700}$
16	$+\frac{1}{8900}$	$-\frac{1}{76000}$	$+\frac{1}{15800}$
17	$+\frac{1}{16900}$	$-\frac{1}{60800}$	$+\frac{1}{18500}$
18	$+\frac{1}{38000}$	$-\frac{1}{50700}$	$+\frac{1}{19500}$
19	$-\frac{1}{152000}$	$-\frac{1}{100300}$	$+\frac{1}{44700}$
20	0	0	0

In this way it is easy to measure not only the maximum value, but the values corresponding to every division of the screw-head. If an objective of high power is employed, care is necessary to have the surfaces of both pieces of glass as nearly plane as possible. Much better results are obtained by using a piece of cover-glass not larger than $\frac{1}{16}$ of a square inch.

Mr. John M. Blake, of New Haven, did me the kindness to photograph on cover-glass, the plate whose measures are given in column III. By reversing the plates, in the way indicated above, he found almost precisely the same value for the maximum periodic error deduced above; viz. $\frac{1}{5600}$ of an inch.

In passing, it may be interesting to note that though the lines of the Rutherford plate are more distinct than those of the Nobert plate, and though the errors of spacing are considerably less, yet the former was rejected from the start as an imperfect one, while the latter gives excellent results, yet both plates will show with about equal distinctness four lines between the components of the magnesium line *b*. This is hardly in accordance with the theory that the optical test of parallelism of lines, and of equality of spacing, is far more perfect than the test of actual measurement. It is evident that the theoretical limit of accuracy required, in order to produce the solar lines in the greatest perfection, has rarely if ever been reached in actual practice. All the evidence seems to point to the conclusion that the brilliancy of the spectrum depends as much on the character of the lines, and especially on the character of the edges, as on the equality of the spacing.

It is obvious, then, that the errors which are to be the most feared, both on account of their magnitude and the likelihood of their escaping detection, are those which are periodic in their character. To the investigation of the sources of these errors, in my own machine, several months of careful study have been given. Without entering into a detailed account of fruitless experiments, I will give only the conclusion at which I have arrived, viz. *that the periodicity resides, not in the screw itself, but in the mounting of the screw.* The evidence on this point seems to be conclusive. In a large number of separate measurements extending over several weeks, substantially the same system of values as those given in column III. were found. These values were also constant for different parts of the screw. Conjecturing that the trouble might arise from unequal friction between the screw and the nut at different parts of the revolution, owing to the want of parallelism between the screw and the fixed way on the bed of the machine a slight movement was given to the adjusting screws, which clamped the split nut. At once the system of corrections was wholly changed, not only in value but in sign, and the values now found, remained constant under every variety of tests applied. After a few weeks,

a slight movement was given to the screws holding the plate against which the precision screw works as a shoulder. The sign of the errors was again changed, but their magnitude was very much reduced, amounting at the maximum to about $\frac{1}{22000}$ of an inch. This system of errors also remained, as long as no further changes were made.

Having definitely found by these and several other similar experiments that the periodicity was not due to the precision screw itself, but to the constrained motion caused by unequal friction between the nut, the screw, and on the ways on which the gravity slide, which carries the plate to be ruled, is moved, I addressed myself to the task of removing as far as possible this source of error. While I have not succeeded with entire satisfaction, the errors of a periodic character have been so much reduced that those which still remain give no serious trouble. By a device to be presently described, these residuals are overcome by an automatic movement connected with the screw itself. Omitting an account of many fruitless trials, I describe the following permanent changes which were finally made.

(*a*) The ways over which the gravity slide moves, one of which was at first $\wedge$ shaped, and the other plane, and both of which were permanently fixed, were both made $\wedge$ shaped and both movable. The ends nearest the point where the bearing of the shaft of the screw works against its shoulder, were pivoted. The other ends were made adjustable with set screws. The precision screw being set in its normal position, and attached to the slide by its nut, the ways are set parallel with the screw by the motion of the slide upon them.

(*b*) The nut, which at first was only about one inch long, was made four inches in length, being one-half the length of the screw. About equally good results were obtained with a lead and a brass nut. The lead nut is much the more difficult to make, as a tap cannot be used. Even when it was cut with a chaser on the lathe, it was found impossible to get a smooth thread until the very simple remedy of keeping the interior wet with a strong soap lye was tried.

The nut having been fitted to the screw, the threads were reduced to a homogeneous system, and at the same time polished, by grinding with the finest emery. It should be remarked that the screw was originally finished in this way, using coarser emery at first. The rule adopted was to grind the screw till all tremor perceptible to the touch in the passage of the nut over the entire length of the screw disappeared.

(*c*) In order to set the screw parallel to the ways in a vertical direction, a hollow cylinder was firmly attached to the under side of the gravity slide. The screw, with the nut upon it, and passing

through this cylinder, was first set in position. The gravity slide having been firmly clamped down upon the ways, the open space surrounding the nut was then filled with plaster of Paris.

In this way the screw is set in perfect adjustment for one position of the gravity slide. Practically, it is found to be in good adjustment for every position upon the ways. But any slight deviation from adjustment in a horizontal direction is corrected by means of the adjustable ways, while that in the vertical direction is for the most part overcome by leaving one end of the precision screw free.

Good results have also been obtained by using a "free nut." In this case nice adjustments are unnecessary, as the nut moves freely upon the screw, pushing the gravity slide before it. If this arrangement is adopted, care should be taken that the nut, if not symmetrical with respect to the screw, should fall freely in the direction of gravity, and bear at every point throughout its whole length against whatever holds it in position while the screw is in action. The most serious objection to this arrangement is a certain amount of lost motion, which seems inevitable.

It is not to be inferred that all periodic errors have been overcome by the arrangement described above; but experience has shown that they have been very much diminished. In fact, I have never succeeded in ruling but two precisely similar plates, in which there was an exact coincidence of every line from beginning to end, when examined under the microscope. In one plate of 100 lines, ruled with great care, each interval being $\frac{1}{2400}$ of an inch, there are, according to three independent measures made by different persons, eighty-four cases in which the errors are less than $\frac{1}{100000}$ of an inch, and the greatest individual error is $\frac{1}{47500}$ of an inch; but the maximum periodic error varies with the different observers between $\frac{1}{20000}$ and $\frac{1}{50000}$ of an inch.

Nobert's bands proceed by increments of 5630 lines to the English inch. The following table gives the number of lines to the inch in each band.

Band.	Lines in an inch.	Band.	Lines in an inch.
1	11259	11	67556
2	16889	12	73186
3	22519	13	78816
4	28148	14	84445
5	33778	15	90075
6	39408	16	95705
7	45037	17	101334
8	50667	18	106964
9	56297	19	112594
10	61926		

How are these Lines accurately spaced?

The ordinary way is to give to the head of the screw, which carries the plate to be ruled, the desired movement over equal intervals by means of a ratchet and pawl: but this method is open to the two objections, that one is limited to the number of teeth cut on the disk, or to an even combination of them; and also, that all errors of the gear-cutter with which the ratchet was originally cut are transferred directly to the rulings, with the addition of other errors arising from want of centering, &c.

I have employed for this purpose the following device, which, as far as I am aware, is new in its application: A rigid arm two feet in length vibrates upon a shaft set exactly in a line with the precision screw. At one end a magnet, fitted to the curvature of the head of the screw, is attached by eight pivots in such a way as to give parallel motion with respect to the arm. The outer portion of the head of the screw consists of a rim of soft iron, which operates as an armature to draw the magnet to it when the circuit is completed. The other end of the arm works between two stops, one of which is adjustable. The action, then, is this: the circuit being completed, the magnet becomes firmly attached to the head of the screw, and by the movement of the arm from one stop to the other, it is carried over a given space. The circuit being broken, the arm during the reverse movement carries the magnet with it without disturbing the precision screw. In order to guard against every possibility of disturbance, a second magnet holds the head of the screw in place while the first one is moving back to prepare for the next increment of motion. By varying the distance between the stops, any desired motion whatever, within certain limits, can be given to the screw. From repeated experiments, it is found that about twenty movements of the arm for $\frac{1}{10000}$ of a revolution of the screw-head can be made without varying more than one from this number.

If now the lower stop is replaced by a wheel made to revolve simultaneously with the head of the screw, and if to the periphery of this wheel a curvature is given corresponding to the known errors of the screw, it is obvious that the screw can be made to correct its own errors. Thus, if at any point in its revolution the screw gives too small intervals, the periphery of the wheel must be filed away enough to increase the space ruled by the amount of the error. I am indebted to Professor Joseph Winlock, the late Director of Harvard College Observatory, for he suggestion of this elegant method of overcoming the residual errors of the screw.

How are Nobert's Lines of Varying Degrees of Fineness ruled on Glass?

First of all, the evidence seems quite clear that they are ruled with a diamond having a knife-edge. In all of the cases which I have examined the lines start in with a curvature, which is maintained throughout the whole extent of the band. I have been able to produce this result only by setting the cutting edge of the diamond slightly inclined to the direction of the line ruled, and this inclination seems to give a decided improvement to the character of the lines.

I assume that Nobert uses a prepared diamond, instead of a natural crystal. It is everywhere assumed by writers on the subject, that only the natural crystal possesses perfect cutting qualities. While this is probably true where a deep cut is wanted for the purpose of fracture, it does not seem to be true where distinct, smooth, and uniform lines are desired. I believe this is also the experience of Mr. Rutherford, who long ago abandoned the natural crystal, either unbroken, or broken into chance fragments. A circular point is objectionable for several reasons, mainly on account of its lack of durability.

Starting with the theory that Nobert's lines are ruled with a highly polished knife-edge diamond, I had constructed from my own designs an apparatus for preparing diamonds in this way.

The machine does not differ from the ordinary tool of the lapidary, except in two particulars; but these are vital to success. It is well known that diamonds can be ground and perfectly polished only in the direction of the cleavage planes, of which there are twenty-four in every perfect stone. A skilful diamond-worker will locate the position of these planes by simple inspection. I found myself obliged to employ the more tedious, but not less sure, method of finding them by a tentative process. The machine was therefore so constructed that the direction of the cleavage planes could be detected after a few trials.

Again, it is customary either to press the lap, on which the diamond dust is placed, up against the diamond, which is set in a rigid holder, or else to connect the holder to a rigid shaft by means of an intervening flat spring. In either case, the diamond is liable to crumble when it is reduced to a sharp edge. In the arrangement adopted, the holder containing the diamond is *free in the direction of gravity only*. This action is secured by two shafts set at right angles, and connected with the required supports by three universal joints. By weighting the horizontal arm or by lifting it with a spiral spring, the pressure can be regulated with great nicety. The lap has a circular movement, while the frame in which it rests has two motions in a horizontal plane, at right angles to each other.

In order to give a motion in revolution to the holder, for the purpose of grinding circular points, a Hook joint is used to connect it with a driving pulley.

It may be proper at this point to offer a few observations, derived from experience, on the kind and quality of glass best suited to receive delicate lines. I have previously made some remarks before the Academy on what, for the want of a better term, was described as the *grain* of polished crown glass. Subsequent observations have not entirely confirmed the views expressed at that time. Still, there does not seem to be much doubt but that certain kinds of glass are capable of receiving perfect lines only in one direction. When the lines are ruled at an angle with the general direction of the grain, the edges at once become serrated if they are very fine; whereas, if they are coarse, they either become enlarged throughout their whole length, the edges remaining smooth, or else they wholly break up, presenting a very ragged appearance. If the lines are as fine as 25,000 or 30,000 to the inch, this delicately serrated appearance can be detected at once; whereas, if the lines are coarse, several days may elapse before the tension by which the particles seem to be held together, is broken.

Two instances occurring in my own experience may serve to illustrate this action. In one, while I was examining a set of lines some days after they were ruled, I was fortunate in seeing two or three lines enlarge throughout their whole length. From being fine lines, they became, almost in an instant, very heavy lines, smooth, black, and of excellent quality every way. The action of breaking up was just slow enough to enable me to follow it. In the other, the lines had been ruled about two weeks; and for protection they were covered with microscopic glass, closely cemented to the surface. During my examination, the whole surface became completely broken up. Such was the force of the explosion, that particles of glass $\frac{1}{1000}$ of an inch in length were driven a distance of $\frac{1}{100}$ of an inch. In fact, the débris covered the whole surface of the glass under examination. All the particles presented a curved appearance; and, with hardly an exception, the curvature was always in the same direction. On both of these plates lines were afterwards ruled in an opposite direction, but without noticeable results.

It may be said that this phenomenon was due to the peculiar action of the cutting crystal with respect to the surface of the glass; but in all subsequent experiments, in which similar but less striking results were noticed, lines were ruled in both directions at the same time, and under the same conditions. In the case of a particular importation of polished crown glass from Chance and Sons, the evidence of grain is so marked and of such constant recurrence,

that all the large plates have been cut into slides 3×1 inch, in the direction indicated by the observations. In general, the direction of the grain can be detected at once by the appearance of lines as fine as 30,000 to the inch, while coarse lines may retain their initial character for several days.

The present indications are that the grain is only surface deep, and that it is the result of polishing in one direction. Common window glass seems to be wholly free from it. Nobert's lines are ruled on microscopic cover-glass about $\frac{1}{200}$ of an inch in thickness. The evidence of grain in this kind of glass is strong; but it is hardly decisive. In some specimens it is very marked, while in others it seems to be entirely wanting. Indeed, any conclusions on this subject must be regarded as only provisional, owing to the extreme difficulty of separating the action of the cutting crystal upon the glass from the effect due to the character of the glass itself. It is, however, safe to say that in certain kinds of glass the best results can only be obtained by ruling in a given direction.

In order to rule bands with lines separated by intervals, e. g. of $\frac{1}{60000}$ of an inch, it is of course necessary to rule single lines whose width is less than this. Great precaution is requisite here, in order to avoid optical delusion. Every microscopist is familiar with the phenomenon of false lines. To avoid errors from this source, a few single lines are ruled between two heavy finding lines. They are then filled with graphite. This precaution is necessary in order to give both visibility and distinctness to the edges. If the lines are not filled, they may appear much finer than they really are; that is, the objective being in focus for the bottom of the furrow may fail to reveal abrasions of the surface on either side. The graphite of the New York Graphite Company will easily fill the finest line that can be ruled with a diamond.

In order to measure the width of the lines, the following plan is adopted as presenting some advantages over the usual method of estimating it by comparison with the known value of a given division in the eye-piece micrometer.

First, a single line is ruled, which in the eye-piece apparently exactly covers the line to be measured under the objective. A few trials will suffice for this purpose. Having found what weight must be applied to the diamond to produce such a line, the next step is to ascertain how many lines, exactly like this one, can be ruled within the space of $\frac{1}{200}$ of an inch with a minimum space between each line. This will also require a few trials. For example, if with a $\frac{1}{15}$th objective and a B ocular, the space $\frac{1}{1200}$ of an inch in the eye-piece corresponds to $\frac{1}{15200}$ of an inch under the objective, and if it is found that fifteen lines can be ruled within

this space, then the width of the line under examination is $\frac{1}{15200} \times \frac{1}{15} = \frac{1}{228000}$ of an inch; a result which is obviously *within* the truth, especially if the line in the eye-piece is made a shade larger than the line under the objective. Tested in this way, the lines of Nobert's 19th band are about $\frac{1}{180000}$ of an inch in width. The photographs made by Dr. Woodward seem to give a little greater value. The finest lines I have succeeded in ruling are about $\frac{1}{160000}$ of an inch in width. These values are substantially the same as those given by Dr. Royston-Pigott, as representing the ultimate limit of visibility under the microscope. The smallest angle at which an object can be distinctly seen is stated by him to be 6″, while other writers place it as high as 60″, or even 120″. Even the smallest value named is much too large. I will at any time undertake to rule a single line, $\frac{1}{30000}$ of an inch in breadth, which can be seen at the distance of seven inches from the eye. This corresponds to an angle of about 1″. In this case the line is filled with plumbago, but if it is reflected from a silvered surface it can be easily seen at the distance of eleven inches from the eye. Comparing minute particles of matter which can be *seen* under a Tolles' $\frac{1}{10}$th objective with those which can be *measured*, in the way indicated above, there is every reason to suppose that the limit of visibility falls beyond $\frac{1}{400000}$ of an inch. It is quite possible that the conclusion reached by Sorby, that the microscope has already reached the limit of its power in *separating* lines whose distance apart is equal to one-half of a wave-length, may be found to be justified by future observations. It is certain that no lines *beyond* Nobert's 19th band have ever been resolved. The great difficulty in distinguishing true from spurious lines has caused more than one skilful microscopist to doubt whether the resolution has been *certainly* carried as far as that point. But that light is "of too coarse a nature" to enable us to see particles of matter as small as $\frac{1}{200000}$ of an inch, is a conclusion which can be refuted without the slightest difficulty.

How are Nobert's Finest Lines produced?

In trying to answer this question, I shall give the results of four distinct lines of investigation. Neither of these furnish conclusive evidence, but they are all suggestive of possibilities.

I. I have already stated that there is strong evidence that they are ruled with a diamond having a knife-edge. To this is added a fact derived from my own experience, and confirmed by a trial of several months, viz. *that when a diamond, having a polished knife-edge, is set slightly inclined to the direction of the lines ruled, its ruling qualities improve with use.* The diamond with which bands of 50,000 lines to the inch were first successfully ruled

would at first barely rule 10,000. It was only after a service of several weeks, its position in the holder meanwhile remaining unchanged, that the highest limit named was reached. Four new diamonds have since been mounted with precisely the same result. It is not to be understood that this remark holds entirely true for heavy lines, such as are requisite for good diffraction plates. It is the experience of Rutherford and others, that one of the chief difficulties in producing such plates is the inability to find a diamond which will do its work equally well throughout the entire process of ruling. But when only very fine lines are desired, the longer the diamond is used, the greater the pressure which can be applied without increasing the size of the line. In this way the lines can be made much more uniform throughout their entire length, than when the diamond barely touches the surface. One can hardly say that the diamond sharpens itself by use, but there is some evidence that the wear is greater on the two faces than on the knife-edge.

When the diamond does its work perfectly, the cut, even of the finest line, produces a sharp singing sound. My ear has become so accustomed to this peculiar tone, that I can judge of the quality of the lines ruled almost as well by sound as by sight. In ruling the highest bands, this sound can be heard throughout the entire length of every line. It does not always have exactly the same character, however, being sometimes much sharper in tone than others.

II. From Mr. Herman, a successful diamond worker of New York, I learned a fact which was thought to be of sufficient importance to justify a somewhat difficult experiment. He stated to me that his experience had shown him that the only really hard points of a diamond are those where the line formed by the intersection of two faces terminates. His directions, therefore, were to grind the faces to a knife-edge, exercising great care to leave the natural line of intersection untouched as far as possible, and then to grind and polish a face nearly at right angles to this line, stopping just at its extremity. He assured me that the success of the experiment would depend entirely upon neither falling short or going beyond this point. Only one diamond has been successfully prepared in this way, and even in this case it is not quite certain that this requirement has been met. Its performance is sufficiently good to warrant further experiments.

III. I am indebted to Mr. D. C. Chapman, of New York, for a third method of preparing a ruling diamond. It is allowed by all familiar with the subject, that the natural face of a crystal is harder than any surface formed by breaking the stone into chance fragments. By splitting a stone in the direction of a cleavage plane, forming an angle of about 40° with this natural face, an

exceedingly sharp knife-edge may be formed, possessing excellent ruling qualities. Moreover, in ruling heavy lines for diffraction plates, the cutting edge retains its form for a long time. In setting the diamond for ruling, the natural face should be slightly inclined to the surface to be ruled. The Brazilian "bort" seems to give the best and most durable cutting edge. With a diamond prepared in this way, the line formed by the intersecting faces being about $\frac{1}{16}$ of an inch in length, I find little trouble in ruling from 60,000 to 80,000 lines to the inch.

IV. A few months since Mr. R. C. Greenleaf, of Boston, placed in my hands a Nobert plate which had been entirely spoiled by the introduction of some kind of fluid between the ruled glass and the slide on which it was mounted. Mr. Greenleaf requested me to undertake the restoration of this plate, kindly offering to assume all the risk of failure. The cover, which had been imperfectly cemented to the slide with something like opal cement, resisted every attempt at loosening. As a last resort, two pieces, about $\frac{1}{10}$ of an inch square, were cut with a diamond from the centre of the cover-glass. After several trials, one of these pieces was cleaned and remounted without material injury to any of the bands. The 19th is quite as easily resolved as in other Nobert plates.

The other piece, being less perfect, was made the subject of a somewhat careful study. Among other experiments, an attempt was made to fill the lines with graphite; but it was found impossible to do so. Even the coarsest lines would not receive and hold it. As I had never before found any difficulty in filling lines either coarse or fine, this result, so entirely unexpected, was noted down as one of which no explanation could be given at that time.

A few weeks afterwards, I succeeded in reducing a black carbon to a knife-edge. Upon an examination of the first lines ruled with it, two facts at once engaged my attention. First, the lines were finer and smoother than any I had ever before ruled. They possessed that quality of glossy blackness which characterizes nearly all of Nobert's lines. Moreover, they seemed to stand out more boldly in perspective than lines ruled with the ordinary diamond. Everyone who has made a study of Nobert's diffraction lines will at once recognize this boldness of perspective as a characteristic feature. Secondly, I was equally surprised to find that the lines *would not receive and hold graphite.*

As these results were confirmed by further observations, it did not seem too much to say that possibly the secret of Nobert's success might consist in his use of a prepared carbon. The natural stone is entirely unfit for ruling purposes.

But it appeared subsequently that this conclusion was too hastily formed, as far as the capability of receiving graphite is con-

cerned. During all these observations, the position of the diamond in its holder remained unchanged; but it was afterwards found that, by giving it a certain inclination with respect to the surface of the ruled plate, it was possible to rule lines, both coarse and fine, which would receive the graphite in the most perfect manner. In general, however, lines ruled with a carbon will take the plumbago perfectly but once. If they are filled and the surface of the glass is afterwards cleaned by rubbing, it is not possible to fill them equally well again. As the filling is not disturbed by mounting in balsam, the better way is to clean the glass thoroughly before ruling, and then mount permanently after the first filling.

Though the carbon is reduced so perfectly to a true knife-edge that the intersection of the two faces appears as a line when examined with an eye-piece of high magnifying power, it is apparent, nevertheless, that the cutting edge is composed of distinct and separate crystals; for in many cases two lines have been ruled at the same time. Generally one is much coarser than the other. Indeed, by regulating the pressure, companion lines can be ruled so fine that it is impossible to see them until they are filled. The setting of the diamond to rule lines of a given kind and quality is simply a question of time and patience. In one hundred trials, perhaps two or three may give lines which will receive plumbago, four or five may give double lines, and one or two may give lines of great delicacy. Great care is necessary in the preservation of the delicate cutting crystal when once found. Notwithstanding the most careful manipulation, it often gives way without visible cause. In several instances, I have been able to locate the exact point where it was destroyed.

In general, the best results have been obtained with the prepared carbon. It is, however, somewhat capricious in its action. The labour of preparation is also much greater than with the African or the Brazilian diamond. The process of grinding occupies from five to ten days. That it is much harder than any other kind of diamond is conclusively shown by the fact that one specimen in my possession has been used in shaping a jewel weighing 180 carats, with only a trifling abrasion of its surface.

In conclusion, I ought to say, in explanation of the somewhat incomplete and fragmentary character of this investigation, that it has been the gradual outgrowth of experiments undertaken for a different purpose. Indeed, whatever has been accomplished thus far may be said to be the result of an unsuccessful search after a spider that would spin a web suitable for the meridian circle of Harvard College Observatory. Failing to find suitable "spider lines," an effort was made to produce on glass, lines of the desired quality and size. This was finally accomplished by etching with hydrofluoric fumes; the lines having been first ruled in a coating

formed by dissolving white wax in gasolene, and uniting the solution by emulsion with liquid gelatine. The coating thus formed will receive lines as fine as 10,000 to the inch, while its protecting qualities are sufficient to withstand very strong fumes. The subsequent experiments detailed in this paper have occupied my attention from time to time during the past three years, when not engaged in my regular duties as Assistant in the Observatory.—*Proceedings of the American Academy of Arts and Sciences.*

PROGRESS OF MICROSCOPICAL SCIENCE.

Diatomaceæ absorbed in their entire state by the Roots of Plants.—Some very curious observations have been made by Professor P. B. Wilson, of Baltimore, U.S.A., which seem to show that the Diatomaceæ when applied to the earth in which corn was grown absolutely passed in their entire condition through the roots and were found in the stems of the corn. In 'Silliman's American Journal' (for May, 1876), Professor Wilson says:—"To demonstrate this theory, my friend G. I. Popplein, Esq., of this city, suggested the application of infusorial earth of the Richmond formation—found in large quantities on the western shore of the Chesapeake bay—to land sown in wheat. I have obtained straw from wheat so grown, and have found, after it has been treated with nitric acid, and the silicious remains placed on the field of the microscope, that it consisted wholly of the silicious shields of Diatomaceæ, the same as found in the infusorial earth, excepting that the larger disks in their perfect form were absent (*Actinocyclus Ehrenbergii* and *Actinoptychus undulatus*). My conclusions are that they, and there probably may be other forms, are too large to enter the root capillaries. During the coming summer I will attempt if possible to make micrometer measurements of both. The discovery of Diatomaceæ in their original form in this wheat straw precludes the possibility of the infusorial earth having undergone any chemical change in the soil, either by forming chemical combination with the alkalies or the earths, or by suffering physical disintegration from any catalytic action of any salts present in the soil. In the particles of silica placed upon the glass slide, when they were completely separated from each other, the outlines of the individual diatoms were sharply and distinctly defined. On the other hand, when the physical action of ebullition with nitric acid was not sufficient for the complete separation of the particles of the epidermal shield, there was observed a marvellous interlacing of the various forms, showing that they were conveyed by the sap-cells directly to the section of the plant where they were destined to complete its structure. I have examined several specimens of straw, taken at random in the market; the silica in each specimen consisted of plates, very thin, and truncated at the corners."

The Markings of Frustulia Saxonica.—Mr. G. W. Morehouse, writing in the 'Cincinnati Medical Journal' of June, says that "Colonel Woodward's observations on this diatom are not accurate." We fancy, however, that the two observers have been treating of different diatoms. Mr. Morehouse says:—"Dr. J. J. Woodward, Assistant-Surgeon U.S. Army, Washington, D.C., publishes an article upon the markings of this fine test-object, in the London 'Monthly Microscopical Journal' of December, 1875. The article is illustrated by six photographs of this diatom. One thing is plainly evident, that however excellent the photographic work may be, it fails in Dr. Woodward's

hands to represent the best work of the microscope. The transverse lines, about 89,000 to the inch, are shown indifferently well, and the finer longitudinal ones are so drowned and obscured as to lead Dr. Woodward to doubt their existence. More careful adjustment and painstaking manipulation, or a better glass,* would have dispelled most of the diffraction lines, lifted the hazy veil, and enabled the observer to see this beautiful shell as others have seen it. It would also have saved him from taking the position of doubting the positive testimony of others when he has nothing but negative testimony himself to offer. The present writer had seen the fine longitudinal lines in question, 95,000 to the inch, counted them, and given the results to the public through the columns of the 'American Naturalist' nearly three years ago, and has seen them many times since. Less than a month ago, as had been the case before, both sets of lines were seen at once, and the face of the shell appeared covered with distinct and regular checker work; an appearance not presented or approached by any of Dr. Woodward's photographs. Both Dr. Woodward and myself were fortunate, or perhaps unfortunate, in having to work on Möller's finest and most difficult specimens. Perhaps Dr. Woodward might have got both sets of markings if he had been as fortunate as was Mr. Hickie † in having coarser specimens to study."

The Structure of a Larval Cirripede.—Mr. Henry Davis, an accurate and careful observer, has noticed some points in the structure of the cirripede, additional to those described by Mr. Darwin in his celebrated work. The original points of his paper ‡ refer to the carapace of this animal. He says:—Darwin speaks of it as being provided with two points at the posterior end, and with a pair of projections, like short horns, in front (which he thinks may be called the ears), but he says nothing of the very noticeable corrugated crest running over the back at the junction of the valves: and, failing any published figures of this larva, some doubt may be attached to the specific name I have appended. The external microscopic structure is very interesting, and has been strangely neglected—only "marks and lines" are recorded; but under a binocular microscope, with a quarter-inch objective and reflected light, we can see the surface covered with deep thin walls or ridges, generally parallel, but in parts tending to confluence; their outer edges are serrated, and the thin walls are strengthened by a sort of buttresses, only seen in certain lights. Towards and over the "ears" the ridges are so modified as to leave hexagonal depressed spaces; while in one spot, beneath which the enclosed larva bears its eyes, the shell is left beautifully smooth and transparent. This creature, then, with its "toughened glass" window, is remarkably well protected; in storms or any danger he has only to shut up his shells, hold on by his antennæ, and keep a good look-out.

* [It is perfectly absurd to suppose that Dr. Woodward was not furnished with the very best glass, and as to careful adjustment that is an equally unnecessary remark.—Ed. 'M. M. J.']

† 'M. M. J.,' March 1, 1876, p. 123.

‡ 'Journal of the Quekett Club,' May.

The Metamorphoses of the Crane-fly and Blow-fly.—A capital paper on this subject is reported in the 'Journal of the Quekett Club' (May). It is by Mr. A. Hammond. The paper is much too long for abstract, but we may state that it deals very fully with the development of the two insects, and that it calls in question some of the views promulgated by Mr. Lowne. The author states that the tendency of his observation is to disprove the distinctive character of the development of the cephalic and thoracic segments in the crane-fly, but to retain it in the blow-fly; and if this be true, seeing that in either case the development originates in structures which are distinctly homologous, the question arises whether the two methods are separated by an impassable gulf, or whether the study of other insects, by revealing the existence of intermediate links between the form, disposition, and connections of these structures in the one case and the other, may bridge over the chasm which at present seems to separate the two methods, and indicate a gradation of modes of development, as well as of external form and internal structure.

A New Diatom.—Mr. A. Cottam thinks that a new species of diatom has been found by Mr. Martin, at Banana Creek, in the Congo River, west coast of Africa. Differences of opinion on this point existed between Mr. Cottam and Mr. Kitton; but, from an examination of Dr. Greville's specimens in the British Museum, Mr. Cottam arrives at the conclusion that the present form is a new species. He says that "the new African diatom appears to agree with *A. Kittoni* in the arrangement of its granulation, although the granules are smaller. It differs from it in generally having a small umbilicus, and in its processes, which, instead of being mammiform, have distinct circular hoods. It appears to him to differ from *A. Johnsonii* in the arrangement and size of its granules (although size is not of much value as a specific distinction), but especially in the fact that *A. Johnsonii* has no raised portions under the processes, and has granules of very different sizes on the same valve. They agree in having an umbilicus, although its presence in the new form is not invariable; and in these too the form of the processes is more alike, although more highly developed in the West African form." He goes at considerable length into the subject in his paper, which must be read for further information.*

The Structure of Connective-tissue Corpuscles is still the subject of debate among the German histologists. In Max Schultze's 'Archiv,' or rather what used to be Max Schultze's, and is now the 'Archiv für mikroscopikische Anatomie,' † is a paper on the above subject by W. Waldeyer. An abstract of his communication is given by the 'Medical Record' (June 15). It is divided under the following heads:—1. *The so-called Flat-cells* ("Platten Zellen") *of the Fibrillar Connective Tissue.*—Under this title the author groups the cells of the loose fibrillar connective tissue and of the formed fibrillar connective tissue of tendons and fibrous membranes. The tendon-cells do not represent simple rectangular plates, but are complicated

* 'Journal of the Quekett Club,' May. † Band ii. p. 176.

structures, which are best characterized as "compound plates," and may be compared to the form of a wheel. A clear conception of the form of these cells is best obtained in the following way: Open a book so that its leaves are kept asunder in groups of four, five, or six, which meet each other at various angles; the whole then makes the same impression as a tendon-cell in miniature. One has not to do with one plate, but with several which are disposed in different ways irregularly one over the other. The margins of these plates are not cut off straight, but project into numerous fine processes, often of considerable length, so that the processes from two neighbouring cells may anastomose; just like the tendon-cells are the so-called fixed cells of fibrous membranes and those of the loose connective tissue. In fact, these cells are neither simple nucleated plates nor spindles, but are "compound plates," of which one, the "chief plate" (Hauptplatte, W.), generally contains the nucleus. The other plates are of smaller size, and appear like little wings, which are attached to the chief plate at acute or almost right angles, and which, just like the margins of the chief plate, send out many small thread-like processes. Where bundles of fibrillar connective tissue are present, they insinuate themselves into the spaces which exist between two plates or wings resting on each other. The cells never lie, however, directly on the bundles themselves, but are always separated by a more or less strongly developed interfascicular, i. e. interlamellar cement from the proper fibrillar mass, so that the cells themselves are buried in cavities in this cement ("Saftraüme," "Juice spaces" of von Recklinghausen). The "elastic stripes" of Boll, according to the author, only represent the side view of a neighbouring plate. 2. *The Fixed Cornea-corpuscles.*—In the 'Handbuch der Augenheilkunde,' by von Graefe and Sämisch, the author has described the cornea-corpuscles as flat bodies, possessing a considerable quantity of finely granular protoplasm arranged around a nucleus which towards the periphery, however, passes into a more homogeneous plate, provided with obvious processes which partly anastomose with those of other cells, and partly end free, so that all the Saftcanälchen are not filled with processes of the cornea-corpuscles. To this description the author adds that the cornea-corpuscles also, like those of tendon and connective tissue, are provided with delicate secondary plates. The nuclei lie in the centre, near the place of junction of the plates; the latter themselves—mostly two or three secondary plates to one chief plate—become quite thin, and are like a veil at the margins, and are there provided with processes. 3. *Large Connective-tissue Cells, rich in Protoplasm.*—Besides the cell-plates, there occurs in the connective tissue another group of cells, not so numerous, but quite as important as these; large, more rounded cells, rich in protoplasm (embryonal cells of the connective-tissue or plasma cells, Waldeyer). They are found sporadically in the subcutaneous connective tissue, and in all fibrous and serous membranes, mostly in the neighbourhood of the blood-vessels. They are distinguished from the wandering cells by their greater dimensions, and by the absence of amœboid movements. Certain peculiar groups of cells, whose histological significance up to this time was not obvious, are nothing

more than groups of these plasma-cells. The author ascribes to this category: 1. The cells of the so-called interstitial substance of the testicle; 2. The cells of the hypophysis cerebri; 3. The cells of the carotid gland; 4. Large round cells, which are found not unfrequently as an adventitious covering on the vessels of the brain; 5. The cells of the suprarenal capsules; 6. The cells of the corpus luteum; 7. The so-called decidua or serotina cells of the placenta. It is characteristic of these cells that they always appear arranged directly around the blood-vessels, which they cover as with sheaths. The author proposes the name of "perivascular cellular tissue" for them.

The Microscopic Structure of Tendon.—A paper by Herr Dr. Herzog is published in the 'Zeitschrift für Anat. und Entwickelung,' &c.,* which is thus abstracted by Dr. Thin in a communication to the 'Medical Record' (June 1876). Dr. Herzog examined transverse sections of the frozen tendo Achillis of the calf. The well-known stellate spaces and their connecting lines (so-called connective-tissue corpusles) appeared dark. The tendon substance enclosed within these spaces and lines, designated by the author a primary bundle, was divided into a number of fields, separated from each other by clear anastomosing lines. In these fields the fibrillæ were seen as dark points. The author succeeded in filling the stellate spaces and their connecting lines with an injection mass, the result being a complete blue network. Upon this Dr. Thin himself observes that the drawing of these peculiar "fields" reminds one at once of Cohnheim's fields in sections of frozen muscle. He believes them to represent transverse sections of the structures he has lately described in tendon as primary bundles (seen lengthways), not to be confounded with the primary bundles of Herzog, which he termed secondary bundles.

Musical Sand examined beneath the Microscope.—A paper on this subject, which is really a somewhat curious one, is published in the last number of the 'Proceed. of Califor. Acad. of Sciences' (vol. v.). Mr. Frink states that "in order to ascertain, if possible, the cause of the sound that is produced by the sand from Kauai, presented to the Academy at a former meeting, I investigated its structure under the microscope, and I think the facts I have ascertained fully explain the manner in which the sound is produced. As the grains of sand, although small, are quite opaque, it was necessary to prepare them so that they should be sufficiently transparent to render their structure visible. This was effected by fastening them to a glass slide and grinding them down until one flat surface was obtained. This surface was then attached to another slide, and the original slide being removed, the sand was again ground down until sufficiently transparent. The grains were found to be chiefly composed of small portions of coral and apparently calcareous sponges, and presented under the microscope a most interesting object. They were all more or less perforated with small holes, in some instances forming tubes, but mostly terminating in blind cavities, which were frequently enlarged in the interior of the grains, communicating with the surface by a

* Band i. Heft 3 and 4.

small opening. A few Foraminiferæ were also met with, and two or three specimens of what appeared to be a minute bivalve shell. Besides these elements, evidently derived from living beings, the sand contained small black particles, which the microscope showed to be formed principally of crystals of augite, nepheline, and magnetic oxide of iron, imbedded in a glassy matrix. These were undoubtedly volcanic sands. The structure of these grains fully, I think, explains the reason why sound is emitted when they are set in motion. The friction against each other causes vibrations in their substance, and consequently in the sides of the cavities they contain; and these vibrations being communicated to the air in the cavities, under the most favourable conditions for producing sound, the result is the loud noise which is caused when any large mass of sand is set in motion. We have, in fact, millions upon millions of resonant cavities, each giving out sound which may well swell up to resemble a peal of thunder, with which it has been compared; and the comparison—I know from others who have heard it—is not exaggerated. The effect of rain in preventing the sound is owing to the cavities in the sand becoming filled with water, and thus rendered incapable of originating vibrations."

A Figure of Bathybius is given by Professor Toula in a description he has supplied of the results and instruments of the sea-exploring expedition of Sir W. Thompson. He has also described some of the Foraminifera. It is to be found in the Annals of the Society of Vienna for the diffusion of Science.

The Development of the Salpa.—This subject has been worked out by Professor Todaro, of Rome, who has presented to the Roman Academy a paper on the organs of generation of the Salpa. He comes to the conclusion that the Salpæ are developed according to the type of vertebrates, being in part like the frogs, in part like birds, and in part like mammals. He says that they represent the trunk of the genealogical tree of the first great division of the animal kingdom.

The Tunicata of the Adriatic have been studied by Professor Heller, who has published an essay on the subject. He describes the internal and external structure of the simple ascidians of this sea. *Ascidia involuta* constitutes a new species. Its body is encrusted with a thick layer of sand, from which only the siphons can be seen projecting. *A. reptaris* is, on the contrary, a naked and transparent body, in the midst of which one can distinguish with ease the ramifications of the interior vessels.

The Organ of Hearing in the Heteropoda.—An important memoir on this subject has been written by Professor Ausserer. It will be found in last year's volume of 'Annales de Gênes.'

A New Parasite has been described before the Society of National Sciences of Pisa by Professor Baraldi. It is the nymph of an acarite *Hypodectes carpophagœ*. This new species has been found in the subcutaneous connective tissue of the pectoral region of *Carpophaga perspicillata*, which died recently at the Acclimatization Gardens of Turin.

Botanical Work at Vienna.—Several important papers appear from the workers at the Physiological Institut of Vienna in the Viennese Journal of Botany. One of these is a memoir on the origin of hairs in the intercellular canals of the mesophyllum of the petioles of *Philodendron pertusum*. Another is on the crystals of oxalate of lime that one sees in the mesophyllum of the petioles of *Pontedera crassipes*. A third is upon the transpiration of the branches of *Æsculus hippocastanum*, of *Taxus*, &c.

The Morphology of the Placenta in Plants.—This subject is worked out in an important memoir, published in the 'Actæ' of the Royal Society of Prague (1875), by Professor Celakowsky. He arrives at the conclusion that the placenta is always a carpellary product, and that it varies only in its points of formation upon the ovary.

The Relations of Algæ and Fungi.—The 'Revue des Sciences Naturelles'* states that in the 'Journal des Sciences Naturelles de Pisa' (fasc. ii.) is published a valuable paper on this subject, by Professor Archangeli. He asks the question whether the gonidia are produced by the frond of lichens (Tulasne's and Nylander's ideas), or are, on the contrary, of an external growth (*provenance extérieure*) (Swendener's view). Accepting the latter idea, he says that the gonidia would form Algæ, and the lichens would become Discomycetous Fungi living upon these Algæ. Professor Archangeli discusses both of these views, but he adheres to the former.

The Sexual Organs of Fungi.—At the late Congress of Gratz, Herr Dr. Eidam related his observations on the sexual organs of Hymenomycetous Fungi. He traced out the development of the male organs of *Agaricus coprophilus*.

Parasitic Fungi.—In last year's volume of the 'Comptes Rendus' of the Academy of Sciences of Naples is a memoir of Professor Cesati dealing with this subject. He treats of *Puccinea malvacearum*, a fungus which does immense damage to the culture of the *Malva rosea* in Bavaria. He also deals with a fungus which produces certain diseases of potatoes, and which is named *Rhizoctoma tubifera*.

NOTES AND MEMORANDA.

A Mode of Centering Mounts.—Dr. Christopher Johnston, of Baltimore, U.S.A., in a short note addressed to us on June 13, suggests the following simple plan for making mounts well centered. After cleaning a slide, choose the better surface for the object. Centre the slide upon a turn-table, with the better side down, rotate the table, and at the same time trace with a pen a circle in ink. This dries in a moment, is an easy guide to the preparer, and can readily be washed off, whatever treatment the slide may receive.

* T. iv. No. 4, 1876.

Resolution of Surirella gemma.—M. Adolf Schultze publishes the following note in 'Science Gossip' for July:—"The resolution of this diatom is not so much a matter of magnification as one of illumination; whether the $\frac{1}{25}$ inch of your correspondent will show the markings depends upon its quality, its correction, and upon the illumination used. A good $\frac{1}{8}$ inch, $\frac{1}{10}$ inch, $\frac{1}{12}$ inch, or $\frac{1}{16}$ inch, will show these markings beautifully, but it is quite possible that the $\frac{1}{25}$ inch will not do so, although it ought. I have, for instance, not resolved the lines into beads yet with my $\frac{1}{50}$ inch, whilst my Ross's $\frac{1}{10}$ inch shows them splendidly. I use the narrow side of the flame of a paraffin lamp, place a bull's-eye condenser with its convex side next to it, and obtain thus parallel rays on the mirror or on the rectangular prism. I always interpolate a blue light modifier. Very oblique rays being essential for the resolution of *Surirella gemma*, a large dark-ground spot or rectangular stop of the condenser must be employed, and the latter, of course, must be racked up rather high. I have, however, obtained the best results by the use of one of Wenham's paraboloids. I put the dark-ground stop flush with its apex, and place this about $\frac{1}{4}$ inch below the object. By changing the position of the mirror or rectangular prism slightly, the true appearance as well as Hartnack's false ones, which Dr. Carpenter has figured, are easily obtained. If the lamp is placed in front of the microscope and the light passed through a bull's-eye condenser directly in the condenser or the paraboloid, the definition is still further improved. The use of monochromatic sunlight facilitates the resolution greatly. Owing to the shape of *Surirella gemma*, only a portion of the frustule can be resolved at one time without altering the focus."

The Aquarium Microscope at Berlin.—The aquarium microscope is a feature which we trust Mr. Kent will introduce at our Westminster tanks. An instrument of this kind has been established at the Aquarium at Berlin by Dr. Zenker. In fact, there are more than fifty instruments in this institution, which are by the well-known makers, Bénèche, Hartnack, Schmidt, and Haensch. Dr. Zenker has, we believe, already given lectures on the different microscopic specimens of the Aquarium by the aid of the oxy-hydrogen lamp and microscope.

Professor Huxley on the 'Challenger's' Work.—At the dinner which was given to the 'Challenger's' staff the other day, Professor Huxley delivered a capital speech, in proposing the health of the scientific staff. Among other things, he said,* "Take, again, the marvellous discovery that over large areas of the sea the bottom is covered with a kind of chalk, a substance made up entirely of the shells of minute creatures—a sort of geological shoddy made of the cast-off clothes of those animals. The fact had been known for a long time, and we were greatly puzzled to know how those things got to be there. But the researches of the 'Challenger' have proved beyond question, as far as I can see, that the remains in question are

* Vide 'Nature,' July 13.

the shells of organisms which live at the surface and not at the bottom, and that this deposit, which is of the same nature as the ancient chalk, differing in some minor respects, but essentially the same, is absolutely formed by a rain of skeletons. These creatures all live within 100 fathoms of the surface, and being subject to the fate of all living things, they sooner or later die, and when they die their skeletons are rained down in one continual shower, falling through a mile or couple of miles of sea-water. How long they take about it imagination fails one in supposing, but at last they get to the bottom, and there, piled up, they form a great stratum of a substance which, if upheaved, would be exactly like chalk. Here we have a possible mode of construction of the rocks which compose the earth, of which we had previously no conception. But this is by no means the most wonderful thing. When they got to depths of 3000 and 4000 fathoms, and to 4400 fathoms, or about five miles, which was the greatest depth at which the 'Challenger' fished anything from the bottom—and I think a very creditable depth too—they found that, while the surface of the water might be full of these calcareous organisms, the bottom was not. There they found that red clay so pathetically alluded to by my friend on the right [Commander Stewart, who replied for the Navy] as the material to which when glory called him he might be reduced. This red clay is a great puzzle—a great mystery—how it comes there, what it arises from, whether it is, as the director has suggested, the ash of Foraminiferæ; whether it is decomposed pumice-stone vomited out by volcanoes, and scattered over the surface; or whether, lastly, it has something to do with that meteoric dust which is being continually rained upon us from the spaces of the universe—which of these causes may be at the bottom of the phenomenon it is very hard to say; it is one of those points on which we shall have information by-and-by."

Comparative Photographs of Blood.—The 'American Naturalist' for May states that Dr. J. G. Richardson, for the sake of illustrating in criminal cases the distinguishable appearances of different kinds of blood, has flowed drops of blood from different animals so nearly in contact on the glass slide that portions of the two drops appear in the same field, and can be photographed together. Dr. C. Leo Mees has modified this method, and obtained exquisite results in specimens presented to the microscopical section of the Tyndall Association. He spreads the blood by Dr. Christopher Johnston's method, which is to touch a drop of blood to the accurately ground edge of a slide, and then draw it gently across the face of another slide, leaving a beautifully spread film. In this way one kind of blood is spread upon the slide and another on the cover. When dry, one-half of each is carefully scraped off with a smoothly sharpened knife, and the cover inverted upon the slide in such position as to bring the remaining portions of the film into apposition. Under the microscope and in the photograph the two kinds of blood appear in remarkably fine contrast, even those bloods that are too nearly alike for safe discrimination in criminal cases being easily distinguished when thus prepared from fresh material.

A Useful Tool for Microscopists is thus described in the 'American Journal of Microscopy' for June. In setting needles in their handles, holding small tools for filing, grinding, &c., holding small pieces of hard material for filing or sawing, and a hundred other operations that every working microscopist requires to perform, nothing is so convenient as a good little hand-vice. The best article of this kind that has come to our notice is one made by the Miller's Falls Manufacturing Company. The jaws move parallel to each other, and are made of well-tempered steel. By means of a notch in the jaws, and a centre point in the main stem, it is easy to grasp any object so that it will be held true, and in this way the vice may be used to hold drills, bits, &c.; and by removing the wooden handle, it may itself be grasped in an ordinary brace. Descriptive circulars may be obtained from the manufacturers, at No. 74, Chambers Street, New York.

CORRESPONDENCE.

Mr. Brown's Paper on Nobert's Lines.

To the Editor of the 'Monthly Microscopical Journal.'

Washington, D.C., *June* 29, 1876.

Dear Sir,—The abstract of Mr. J. A. Brown's paper on the Nobert's lines, in the June number of your Journal, was the means of directing my attention to the original paper "On the Power of the Eye and the Microscope to see Parallel Lines."* It appears from the original paper, that the photographs so elaborately measured were a set of paper prints made at the Army Medical Museum, and sent thence to Mr. Eulenstein, of Dresden, in 1868. These photographs were the work of my former assistant, Dr. E. Curtis, who, through a misprint, figures in Mr. Brown's paper as "Dr. E. Carter, Surgeon of the U.S. Army."

These photographs of Nobert's lines were described by me in a paper published in the 'Quarterly Microscopical Journal,' October, 1868. I there expressly declared that the photographs of the 16th, 17th, 18th, and 19th bands showed spurious lines only. All that Mr. Brown has so laboriously measured on these bands, as shown in the photographs, is therefore without any value as a measurement of the actual rulings of Nobert. I may say of his other measurements, that I can only regret that so much labour has been lavished upon paper prints, which are never of the same size as the original negatives, from which they always differ (after washing and mounting), both in magnifying power, in the thickness of lines shown, &c. To have any serious value as a study of the actual rulings, these measurements should have been made on the original negatives, or at least on glass contact prints. Should Mr. Brown be willing to undertake

* 'Proceedings of the Royal Society of London,' vol. xxiii. 1875, p. 522.

their study, I would take pleasure in sending him a series of such prints on glass, to which I would also add some which give the true resolution of the higher bands. I think he would then arrive at conclusions differing in important particulars from those which he has expressed.

Very respectfully,

J. J. Woodward,

Assist.-Surgeon U.S. Army.

The "Bramhall" Reflector, or New Oblique Light Illuminator.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—I have much pleasure in calling the attention of those interested in the resolution of the striæ on the finely lined forms of Diatomaceæ to a simple form of apparatus invented by the Rev. — Bramhall, of Lynn, which after a careful trial I am able to say is superior to anything I have hitherto had the opportunity of trying. This illuminator in its simplest form consists of a disk of silvered glass about one inch in diameter, mounted in a wood or brass fitting similar to a selenite stage; the disk should be sunk in it about one-eighth of an inch. Mr. Bramhall informs me that with sunlight it resolves striæ far better than any other mode of illumination. My own experience has only been with the ordinary micro lamp; this requires to be elevated from three to four inches above the stage, and the light, after passing through the large bull's-eye condenser, should impinge on a smaller one placed close to the stage, the height of the lamp from the surface of the condenser of course regulating the obliquity of the reflected ray.

The performance of the reflector is improved if, instead of being constructed of silvered glass, a disk of speculum metal is substituted, and in place of the supplementary stage the reflector should be mounted on an adapter fitting into the sub-stage of the microscope; by this means the distance from the lower surface of the slide can be regulated.

This illuminator can also be used in place of the "Spot Lens" with the lower powers, but a short tube to slide on the objective is necessary to prevent the reflexion from the upper surface slide passing into the objective. I find that the tube attached to the Lieberkuhn (the reflecting portion being detached) answers very well.

Careful adjustment of the objective is necessary, and the object should be perfectly parallel with the stage of the microscope.

Yours very truly,

Fred. Kitton.

P.S.—Mr. C. Baker, 244, High Holborn, London, has undertaken to supply the Bramhall reflector.

PROCEEDINGS OF SOCIETIES.

Microscopical Society of San Francisco.

The regular meeting of the San Francisco Microscopical Society was held on Thursday evening, May 18, as usual, with a good attendance of members present.

Mr. Kinne read an interesting paper on a new variety of acarus found in the ulcerous root of the lemon and orange trees, and exhibited the living animal, with the same mounted, to show the various parts, after treatment with carbolic acid to render them transparent. Several important differences were noted from others of their kin, which the prepared specimens and an enlarged drawing on the black-board made quite plain.

Mr. Hyde offered a plan to be observed in the selection and arrangement of objects for the annual reception, which, slightly modified, was adopted.

The fourth annual reception of the San Francisco Microscopical Society was held in Mercantile Library Hall, on Thursday evening, May 25.

The regular meeting of the San Francisco Microscopical Society was held on Thursday evening, June 1, President Ashburner in the chair.

Mr. C. L. Peticolas, of Richmond, Va., sent the Society a finely mounted slide of fossil diatoms from that place.

Mr. H. G. Hanks donated a slide mounted with diamond boron, which he had prepared in his laboratory. Mr. Hanks describes this very interesting substance as an element of which boracic acid is the teroxide. Like carbon, it assumes three allotropic forms, the *amorphous*, the *graphoidal*, and the *crystalline*, or diamond boron. It is the hardest of all known substances, scratching even the diamond itself.

Mr. J. Edwards Smith, of Ashtabula, Ohio, corresponding member, sent the following:—The Cincinnati 'Medical News' for May, 1876, contains a description of two amplifiers by the Rev. J. H. Wythe, M.D.

Dr. Wythe informs your Society that during the past two or three years he has made many experiments, resulting in the discovery of two amplifiers, both of which he describes—I quote Dr. Wythe's own words: "The second form of amplifier is better still, and consists of a double concave lens, having a virtual focus of about one and one-half inch, at the end of a tube about 6 inches long, at the other end of which is the ordinary negative eye-piece."

I have in my possession exactly the amplifier above described, which was made for me by Mr. R. B. Tolles seven years ago, and it has been in almost daily use. My attention was first called to the amplifier from seeing an advertisement of it by Mr. Tolles, and it must have been some two years afterward that I sent to Mr. T. my

order. Hence it is probable that Tolles has made these amplifiers for the last nine years.

I am entirely in harmony with Dr. Wythe's general position, to wit: "That future progress in the power of the microscope must depend on the eye-piece, or intermediate arrangements of lenses between the eye-piece and object-glass."

Dr. Wythe, who was present, remarked that he had not been aware of such an amplifier as his ever having been made before; and Mr. Hyde stated that he had used one of Tolles' amplifiers for some years, and it seemed hardly possible that they could be "exactly alike," for the results regarding magnification and definition were not at all the same, and that Tolles' does not approach the Wythe amplifier.

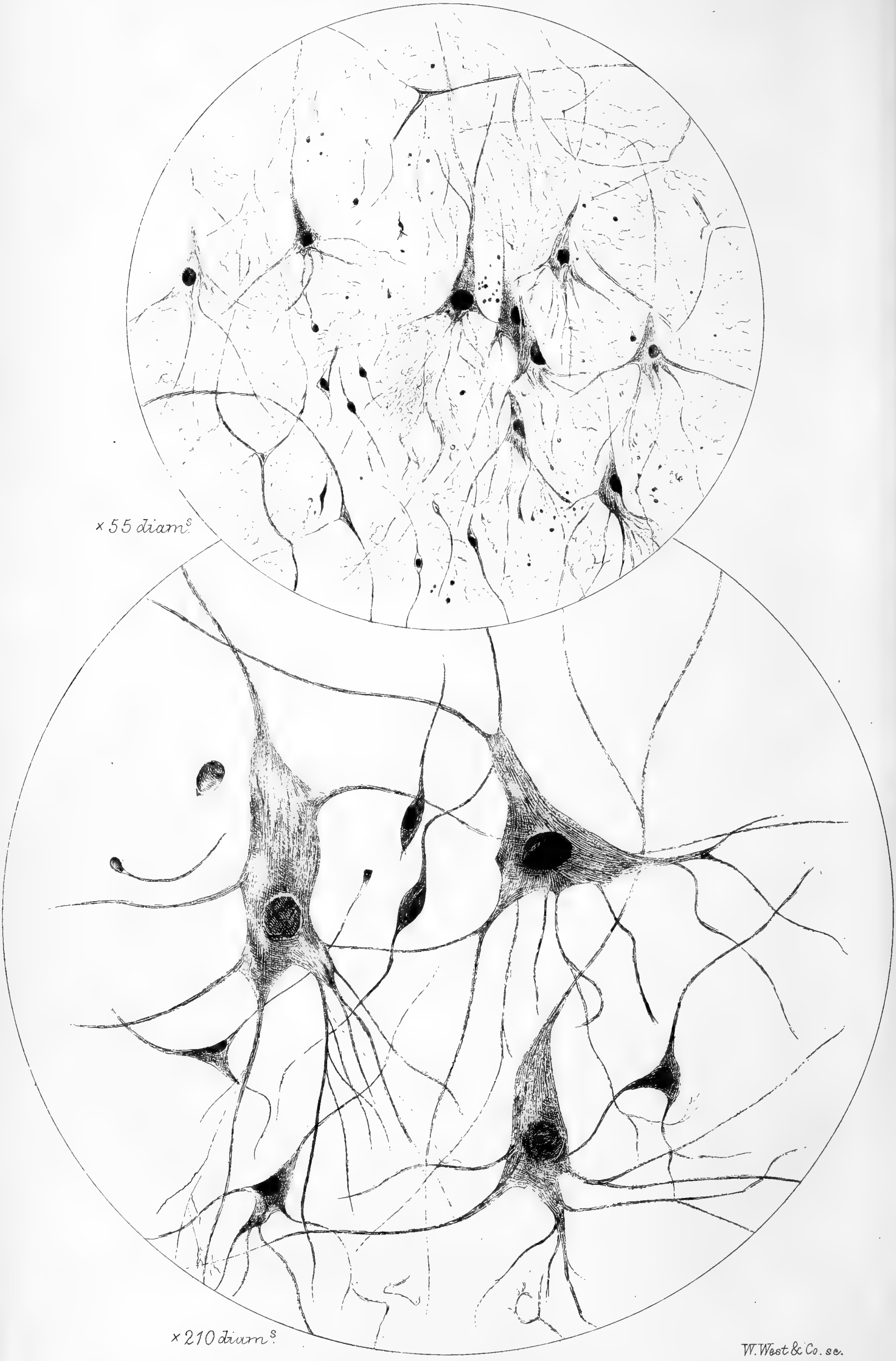
The regular meeting of the San Francisco Microscopical Society was held on Thursday evening, June 15.

Mr. J. A. Langstroth presented two slides mounted by him with the transverse section of the ovary of *Tropæolum majus*, and the pollens of convolvulus, honeysuckle, pansy, and others, fixed on the same slide, in order to readily compare their size and shape, both of which slides, on examination, were found very interesting.

Gen. Hewston donated a slide mounted with volatilized gold, which, under a $\frac{2}{3}$rd objective, opaque, was not only a beautiful, but instructive object. The microscopic globules were perfect in shape, and were obtained at some distance from the melting pot, from which they had been thrown off by the draught and heat in a volatile form, so to speak, and condensed in the air in the form of minute shot, forming a veritable shower of golden rain. With all the care and appliances for the prevention of wastage in smelting or refining gold, a portion is lost in this way; and no doubt the roofs of the houses adjacent to mints and refineries would yield enough of the precious metal to show the colour, at least under the microscope.

The fact that the Society's Nachet No. 5 objective had shown No. 19 of Möller's test-plate into beads, at a former meeting of the Society, and had attracted considerable attention from some microscopists in the East, was alluded to by Mr. Hyde, and in connection he made a statement to the effect that the test-plate used on that occasion was one of a dozen which had been but recently sent out from London, and has proved to differ from all the rest, and others owned by members of the Society, in the respect that not only this No. 5, but any good glass of $\frac{1}{5}$th and upwards, could resolve No. 19.

Mr. Hyde further stated that he has examined the diatom carefully, after securing the slide as a curiosity, and has no hesitation in stating that it is a true *Nitzschia curvula*, but with the peculiarity, amounting to almost an anomaly, that it is so easily resolved. The No. 5 fails to resolve No. 19 on the other slides; and while it is an exceptionally good glass, it has, by a curious combination of circumstances, provoked considerable discussion—not its fault or the observers of the dots, but attributable to the fact that the anomaly was in the object and not the objective.



× 55 diam^s

× 210 diam^s

W. West & Co. sc.

Human Brain—Ascending Frontal Convolution.

THE

MONTHLY MICROSCOPICAL JOURNAL.

SEPTEMBER 1, 1876.

I.—*A New Process of Preparing and Staining Fresh Brain for Microscopic Examination.* By Bevan Lewis, F.R.M.S., Pathologist and Assistant Medical Officer, West Riding Asylum.

(*Taken as read before the* Royal Microscopical Society.)

Plate CXLIX.

It has always appeared to me a matter of very essential importance that the microscopical examination of the brain should involve some process applicable to immediate use, and that we should not, as is so often the case, rest content with its microscopic appearances until the lapse of six or eight weeks has prepared the brain substance for examination by the ordinary processes of hardening and section cutting.

The earlier our examination is made after death the more probable is it that minute lesions will be detected and fallacies eliminated which are necessarily involved in processes more complicated and prolonged. In advocating a method which I now adopt for the examination of fresh brain I would wish my readers to fully understand that I do not for an instant assume that any such process can possibly supersede the now venerable method of hardening by chromic acid and its salts. Both methods should be employed in all cases, for each has its individual merits; thus the older plan preserves to us most faithfully the natural *relationships* of individual parts, which cannot be said for the new process; on the other hand, we gain by the fresh examination far greater *details* of structure and *differentiation* of the various constituents, besides other advantages naturally attendant upon so early an investigation. It will therefore be evident that whilst for researches in comparative histology the hardening reagents must be employed, for many purposes, especially *pathological*, the fresh method has its special advantages. In the pathological department of West Riding Asylum I have enjoyed especially good opportunities for thoroughly testing the question whether or not a process could be devised for examining in the fresh condition the structure of the cerebral and cerebellar cortex. I have no doubt that in this investigation I am working side by side with others equally anxious to solve the

question, and trust that the publication of my own results may, whilst it tends to prove that at least some advance has been made in the right direction, also serve as a stimulus and encouragement to the prosecution of what certainly is a praiseworthy object. Dr. Batty Tuke certainly has the merit of first applying the process of staining to fresh brain, but the method employed by him differs in many very essential points from the process which I have here to recommend. I cannot undertake to express any opinion on the merits of his process as I have not myself adopted it, but I have been told that it occasionally yields very good results. The aniline black which is so valuable an adjunct in staining the nerve-cells was not employed by Dr. Batty Tuke, attention having been directed to this reagent by Mr. Sankey last year.

Another more recent mode has been adopted by Mr. H. R. O. Sankey, and deserves special attention, as by its aid the most beautiful preparations may be obtained. His process, briefly described in the pages of the 'Lancet,' was afterwards made the subject of an able paper in West Riding Asylum Reports for 1875.

Some months ago I described in the 'Medical Times and Gazette' a rough and ready method for examining the brain during an ordinary post-mortem, by which method the varied histological constituents may be well differentiated and morbid textural changes at once detected. The difficulty of obtaining a fair film by this measure may be objected to by some, but as the process almost entirely depends upon manipulative dexterity, any practised microscopist will find no impediment in his way. The method, however, which I have now to describe is developed out of the sum total of my experiments, and as it invariably presents us with a beautifully delicate film independent of any special amount of manipulative skill, it offers peculiar advantages to the most inexperienced worker. For facility of description I will arrange the subject under three heads embracing three stages of the process, as follows:—1. The preparatory stage. 2. Staining and differentiation. 3. Permanent mounting.

First, or Preparatory Stage.—The convolution of which the structure is to be examined is stripped of its membranes and a portion excised of a bulk sufficient for convenient manipulation. It is held between the thumb and second finger of the left hand, the index finger being utilized as a support and guide for the blade. One of the larger razor section-knives 6 to 7 inches long by 1 to $1\frac{1}{2}$ inch in breadth will be found most convenient for making these sections. The upper surface of the blade should be deeply concave and kept deluged with spirit. A clean sweeping cut should now be made perpendicularly to the cortical layers down through the white matter, passing from without inwards so as to expose a perfectly smooth even surface. Thin vertical sections of the cortex

and a small portion of the medulla are now to be made by the same sweeping cuts, the precaution being observed of keeping the surface operated upon as well as the blade thoroughly and constantly moistened with spirit. Extremely delicate sections may thus be made after a little practice by rapid steady strokes of the knives, and all tearing avoided by the floating up of the section upon the moistened blade. I usually operate upon half-a-dozen sections simultaneously, as no further trouble is involved in preparing a large than a small number of such specimens. Having therefore the slides well polished placed before us, the finest and more delicate sections are selected and placed on the glass slips and a few large drops of Müller's fluid dropped over each section from a glass pipette or camel-hair brush. This fluid is allowed to thoroughly deluge the section above and below for some seconds, and a circular cover-glass dropped on in such a position that the section occupies about one-third to one-half the diameter of the cover. The covering glasses most serviceable for slides measuring 3 by 1 should be quite 2 centimeters in diameter. The blade of a knife or point of a strong mounted needle may next be placed on the centre of the cover and by steady gentle pressure the grey matter is flattened out into a thin almost transparent film. Practice soon enables the operator to so adjust the position of the cover that the extreme edge of this film still occupies the space covered by the circle. It will, however, be found of very slight disadvantage should the film be partly pressed outside the cover-glass, although I generally avoid such an occurrence, as it is not so likely to lead to the production of a film of uniform thickness. The superfluous fluid is removed by rapid rinsing in water, and the slide next transferred to methylated spirits. For this purpose I use a flat porcelain bath such as is employed by photographers, and which should contain sufficient spirit to cover the slides. In from thirty to forty seconds the film will be found in the best condition for removal without tearing. For this purpose the slide is removed, one edge of the cover-glass steadied with the finger, and the blade of a penknife gradually inserted beneath the opposite edge, and it will be found most advantageous to elevate that edge which impinges on the lower cortical layers or on the medullary substance. The cover is in this way most readily removed, and the most perfect films remain either loosely floating on the glass slip or closely and smoothly adherent to the cover. For rapid staining it is now advisable to remove all traces of spirit, and this is most readily accomplished by slightly inclining the slide or the cover to which the film is attached and allowing a gentle stream of water to flow from a large camel-hair brush over it. Having passed through this preparatory stage the film is ready for the second stage, that of staining.

Second Stage.—The film is now to be subjected to the agency

of some of the more suitable staining reagents, and it is necessary to remark that it is at present in the condition most suitable for histo-chemical research; the slightest tinting with a weak carmine solution just sufficient to render the cell elements visible enables the microscopist to apply his reagents to great advantage; at the same time metallic impregnation may be employed, as by the salts of gold, silver, platinum, or the hyperosmic acid. The dyes which I usually employ are the aniline and carmine solutions. The former as a 1 per cent. solution of aniline black is pre-eminently useful for staining the cells of the cortex and their processes, whilst carmine deserves the preference for the best differentiation of the delicate neuroglia basis or framework of the nervous system. The staining with aniline is thus accomplished: a large drop of the solution is placed on the film, which in a short time begins to assume a general and decided coloration. When the requisite colour has been acquired (a matter learned by experience only) the slides are transferred to a vessel containing water, and *very gently* lowered and allowed to rest on the flat bottom of the vessel. This immersion favours the staining and differentiation, for it will be observed that, as the superfluous dye floats away, aided by the gentle swaying to and fro of a brush in the fluid above, the film becomes deeper in hue and the staining perfectly uniform. The slide is now as carefully removed, all fluid drained off, and the preparation transferred to a bell-glass which protects it from injury and dust during the process of drying which it has next to undergo prior to mounting.

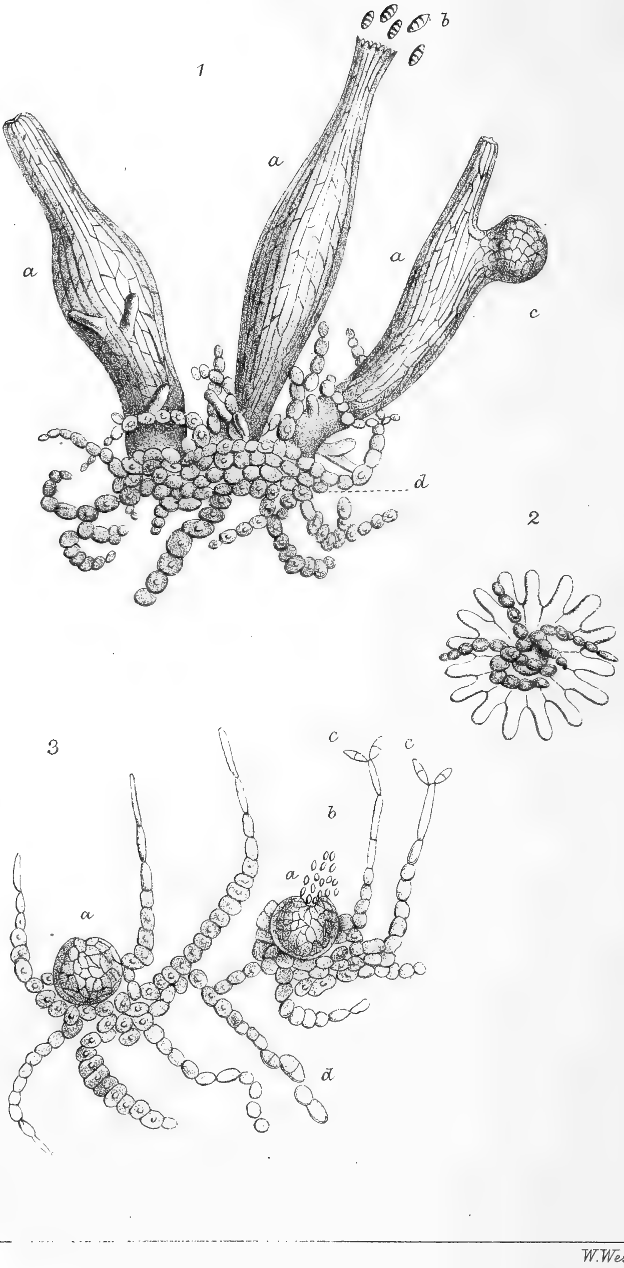
Third Stage.—Before the specimen can be permanently mounted in balsam it must necessarily undergo complete dehydration. For this purpose three methods present themselves for our choice, viz. 1. Spirit. 2. Artificial heat. 3. Spontaneous drying beneath the bell-glass, with or without the agency of concentrated sulphuric acid. The first method I have now completely discarded, as presenting numerous objections. The employment of artificial heat is also as far as possible to be avoided except where time is a matter of great import. The latter method is according to my own experience decidedly the best, and the addition of a small vessel of concentrated sulphuric acid beneath the bell-glass covering the slides very materially hastens the drying without in any way interfering with the delicate structures exposed in the slides. When perfectly dry a few drops of chloroform are placed over the film, which if examined in this moistened condition displays exquisitely the minute structure of the cortex. The chloroform must not be allowed to remain any length of time, a few seconds sufficing, and ere evaporation occurs a drop of a benzole or chloroform solution of Canada balsam is allowed to fall on the preparation, the cover-glass adjusted and gently pressed down. Oil of cloves may precede the

balsam in lieu of chloroform, but judging from the results obtained I find the plan advised preferable.

In reviewing briefly the process now described it will be seen that the preparation of the film is a feature of most essential importance, and it will be readily understood that the employment of Müller's fluid can prove in no way detrimental to the delicate cellular apparatus with which we are dealing. This fluid medium has been, I believe, very universally acknowledged by most histologists as the best for the preservation of the most delicate nervous textures, such as the retina, &c., with the least possible alteration of textural relationships. By its means, also, we avoid the extreme friability which a film produced in water alone acquires, and at the same time we reduce to a minimum that adhesiveness natural to a thin lamella obtained from the brain by pressure, and hence render the removal of the cover-glass a matter of the greatest simplicity. The subsequent short treatment by spirit still further aids the last-mentioned object, and serves to free the film rapidly from all traces of the bichromate. Thus, then, is our primary object attained in the formation of a delicate lamella of the cortex, and the safe removal of the covering glass. Once this is obtained, we need no longer fear the friability induced by endosmose of water, and which would have been fatal to our object if induced prior to removal of the cover. The subsequent removal of spirit is found essential to a perfect and rapid staining by the aqueous solutions of aniline, and the further steps of the process explain themselves, I believe, too readily to our minds to need any further comment upon.

Those who have attempted the staining of fresh as well as hardened brain will readily allow that the dye acts far more energetically and with more favourable results in the former than in the latter case, and differentiation is ensured by the absence of that diffuse staining which so often complicates matters in the preparation of sections obtained from brain hardened by chromic acid. The idea therefore occurred to my mind that if hardening by chromic acid and its salts acts thus detrimentally in the subsequent staining process, could we not devise a method whereby the staining might be completed ere this unfavourable effect upon the germinal centres took place? I believe I have solved this question satisfactorily, for I find that staining of a most perfect character may be induced during the subjection of fresh brain to certain hardening reagents. Some of the best carmine preparations of the cortex which I possess were obtained by this method, and I do not hesitate to affirm that I believe it to be a decided improvement upon the older method. Time and space preclude me from giving even a brief outline of this process, but I can assure my readers they will find an adjunct of extreme utility in a mixture of Müller's fluid and carmine dye of the original strength given by Beale's formulæ. By the time that

hardening has proceeded sufficiently far for section cutting, it will be found that a very perfect and beautiful staining of all the layers of the cortex has been acquired, and the sections have simply to be washed, dehydrated, and mounted by Clarke's method. It will probably occur to the reader that if Müller's fluid does not interfere with the staining by carmine or aniline, a further simplification of my method of fresh-brain examination would be possible by carrying on the staining simultaneously with the preparation of the film, and so reducing the two stages to one. That this is possible I allow, but since the method I have described yields the best results, I have no hesitation in recommending it above all others for trial. The process of fresh-brain preparation can scarcely be called a crushing of the cortex, as I have heard it designated—this term decidedly originates a wrong impression. The neuroglia basis of the brain is of a very resilient character, and it is really surprising to what an extent its structure may be subjected to pressure without rupture and tearing of the nerve-cells and processes. If we assume with Gerlach that one of its essential constituents is purely elastic fibre, as seen in the neuroglia of the cord, this explains its peculiar immunity from injury under such rough usage, and the result is seen to be equivalent to the most delicate teasing of tissue, the processes being gradually unravelled from their dense networks, and the structural elements universally displayed to the best advantage. The accompanying Plate may serve to illustrate the appearance of the larger and smaller pyramidal cells of the cortex as obtained by this process. The sections (Plate CXLIX.) are from different portions of the ascending frontal gyrus, and are magnified 55 and 210 diameters respectively by an inch and a quarter-inch objective of Smith and Beck with No. 1 eye-piece.



W. West & Co. lith.

Fungus in the orange & olive.

II.—*On a Disease of Olive and Orange Trees.* By W. G. Farlow, Assistant Professor of Botany in Harvard University, U.S.A.

Plate CL.

Last summer numerous complaints came from southern California of a fungus which had attacked the olive and orange trees, and which was causing a considerable loss of those two crops. Our attention was first called to the subject by Dr. H. W. Harkness, who, in a letter from San Francisco, dated May 11, 1875, sent a specimen of the fungus on an orange leaf from southern California. Of the extent of the ravages of the fungus at that date no information has been received; but as, in a letter from San Diego, dated June 3, 1875, Mr. D. Cleveland wrote that there was no trace of the fungus in that vicinity, we may suppose that the disease first appeared not far from Santa Barbara, where we have definite knowledge of its occurrence, and where great damage was done later in the summer. In a letter from Dr. George Thurber, dated September 20, enclosing some specimens of the fungus, is the following from a correspondent in Santa Barbara: "We are troubled with our olive, lemon, and orange trees. A small fungus appears on the leaves, twigs, and branches, at first visible only with a microscope, and of a green colour. As it increases in size it turns brown, and then black. The olive is so exhausted that it is unable to fruit. The orange and lemon stand it better, but their fruit is so inferior as to be practically worthless." On the day of the receipt of Dr. Thurber's letter, another was received from Professor Dana, also enclosing specimens from Santa Barbara.

From the general tenor of letters from California, it is evident that, if this is not the first year of the appearance of the disease, it is, at least, the first in which it has attracted general attention; that its effect on the olive and orange crops has not been slightly but markedly injurious; and that, in its advanced stages, there is present on the leaves and stems a blackish substance, which is universally regarded, by those who have formed any opinion on the subject, as a fungus. We have received, at different times, from California specimens of leaves and stems of orange and olive trees covered with the black growth, and have been able to study the fungus, which presents some points not without interest in a botanical point of view; and if our conclusions do not point to a direct remedy, it will be conceded, we hope, that we have contributed toward removing some misconceptions as to the nature of the disease. At this distance, remote from all opportunities of observing the disease on living trees, there are of course some points in the development of the fungus which we have not been able to study; and our

correspondence has not been sufficiently extensive or minute to enable us to give any statistics of the ravages of the disease, to ascertain the climatic or other changes which have preceded or accompanied the breaking out of the epidemic, or to decide whether it is the same form of disease which has been reported to occur in Florida. Our specimens present the disease as it appears when in a somewhat advanced stage, and after the leaves and stems have become so changed as to attract attention.

Mycelium.—The leaves of the olives which are affected by the disease are somewhat curled and shrivelled, and are of a browner colour than normal leaves which have been gathered but a few weeks. On both surfaces of specimens sent us are black spots of greater or less extent, but in no case is the leaf perfectly black. On the upper surface the black spots are more numerous, more distinct in outline, and harder in substance, than on the lower, where they were more diffuse and of a powdery consistence. The twigs, of which we received only small specimens, are covered with spots which resemble more closely those on the upper than on the lower surface of the leaves. In one specimen the spots are nearly confluent, and the bark is visible in only a few places. After the leaves or stems have been soaked in water for a short time, the black substance can be scraped off without the least trouble, leaving the bark tolerably clean. The black substance, when seen with a magnifying power of four hundred diameters, is found to be composed of the stellate hairs peculiar to the olive, over which is growing a fungus, to the dark colour of whose mycelium the spots owe their colour. The mycelium is very variable in appearance. As a rule, it is composed of moniliform hyphæ, whose cells are ·006 mm. by ·008 mm., and in some places almost spherical. The colour of the cell-wall is a dark or purplish brown, and in most of the cells there is a comparatively large-sized oil-globule. These hyphæ branch in all directions, and the cells of the branches grow constantly longer, narrower, and paler, although in all cases retaining a tinge of brown. The relation of the mycelium to the stellate hairs and outer part of the twigs and leaves is clearly seen in cross sections. The hyphæ run along the surface of the epidermis and of the hairs, which it will be remembered resemble a broadly-opened, short-handled parasol. They are twined closely round the stems of the hairs, so closely, that the fungus cannot be removed without tearing them off. They do not enter into the cells of the olive, and there are no haustoria as in the case of some of the leaf parasites belonging to the *Erysiphei.* Occasionally there are little knob-like projections of the cells which seem to indicate haustoria; but by the most careful examination which we have been able to make, we

have not been able to see that they enter into the cells of the stellate hairs or epidermis and act like haustoria. The surface of the hairs and epidermis, however, seems covered with a sticky substance (of which we shall have more to say hereafter), to which the hyphæ closely adhere. Plate CL., Fig. 2, shows one of the stellate hairs seen from below, with a portion of the mycelium growing upon it.

Various modifications of the mycelium are found principally on that portion growing on the outer part of the stellate hairs exposed to the air. After reaching a certain stage of development, they grow together in such a way that the hyphæ coming together laterally form a sort of membrane, as shown in Plate CL., Fig. 1, *d*. This membrane is composed of only one thickness of cells, but is very uneven as it follows and conforms to the inequalities of the hairs. Its general direction is parallel to the surface of the leaf or stem on which it is found.

Conidia.—The hyphæ at their free ends branch in all directions, and bear reproductive bodies of several kinds. The simplest form is that shown in Plate CL., Fig. 3, *d*, where the ordinary cells of the mycelium divide by cross partitions into two parts, which do not respectively grow to the same shape as the mother cell, but remain together two by two, as shown in the figure; the hypha becoming zigzag by the alternate lateral displacement of the pairs of cells, which finally drop off and readily germinate, each cell producing a germinal tube. In other parts of the mycelium, the terminal cell of certain threads divides by means of partitions, parallel to and at right angles to the axis of the filament, until a compound body is formed, which resembles the spores of the so-called genus Macrosporium. These bodies, which can only be described as irregular conglomerations of cells of an oval outline, are produced in great abundance and average ·015 mm. by ·025 mm., but are often much larger, though often smaller. They easily drop from their attachments and germinate, each cell being capable of producing a germinal tube. Other hyphæ, rising at right angles to the plane of the membranous portion of the mycelium, grow more and more attenuated, and branch at the tip; the terminal cells divide in two, as in Plate CL., Fig. 3, *c*, fall from their attachment, and germinate. This last modification of the hyphæ, which is by no means so common as the two previously described, will be recognized as corresponding to the so-called genus Helminthosporium, or Cladosporium, if we examine before the terminal cells have divided. It is out of the question to give specific names to such forms as those just described, which, since the publication of Tulasne's 'Carpologia Fungorum,' are known to be different states of development of species of Pyrenomycetes.

Pycnidia.—Besides the forms already described, there are

other bodies of a more complicated nature. Plate CL., Fig. 3, *a*, *a*, represents the *pycnidia*, which are quite numerous in the spots, both on the leaves and the stems. Their general shape is spheroidal. They consist of a membranous sac of the same colour as the darker parts of the mycelium, in which are contained the small bodies, which are represented as being discharged in Fig. 3, *b*. Their average diameter is ·04 mm. In general appearance the pycnidia resemble so closely those with which every-one is familiar in other Pyrenomycetes, that any further description is unnecessary.

Stylospores.—In examining the larger black spots on the stems of the olive, other bodies are seen,—the *stylospores*, to adopt Tulasne's nomenclature. They are represented in Fig. 1, *a*, and resemble flasks, whose long necks project beyond the mycelium, by which they are surrounded. They may be recognized by the naked eye, and clearly seen with a hand-lens, as the black projecting necks are tolerably conspicuous. To obtain a good view of them, some of the larger black spots must be picked to pieces, and the fragments treated with caustic potash, and afterwards hydrochloric acid. The shape of the separate flasks is quite variable. The central portion of Fig. 1 represents one of the more regular, where, starting from a somewhat contracted base, there is a regular swelling of the central portion, which again diminishes into a rather long neck of uniform size. In some cases, the flask, instead of being straight, is flexuous with two swellings, the upper one being smaller than the lower. Others, still, fork, and usually one branch is much more obtuse than the other. The size of the flasks varies very much; but even in their younger states they can generally be distinguished from the pycnidia, by being less inclined to a spherical shape. The height is as variable as the outline. Some of the smaller are ·15 mm. high; others—and they are nearer the average—are ·4 mm. The wall of the flasks is composed of dark-coloured cells, which are longer in the direction of the axis of the flasks.

In some cases the cells composing the wall of the stylospores grow outward, so as to form papillæ; and as the mycelium at the base generally sends up branches around the flask, it is only by a careful dissection that the base can be clearly seen. At first the mouth is closed, and there is a depression of the cells at the centre; but later they spring back so as to form round the open mouth a circle of slightly reflexed teeth, whose tips are perfectly hyaline. The neck of the flask is hollow; but in the swollen portion spores are borne. They are oval, and divided into four parts by cross partitions. They are not contained in asci, but are attached to short filaments which line the surface of the base and lower portion of the sides of the flask. They escape readily through the open

mouth; and slight pressure on the covering glass generally causes a fresh discharge.

So far, we have spoken of the fungus as seen on the olive. The orange leaves sent us are also covered with a black substance, which is not so much in spots as in powdery sheets upon both surfaces of the leaves, more particularly the upper. The attachment to the leaf is by no means as strong as in the olive; and the deposit can easily be scraped off, even without previous moistening. In fact, in some places it falls off on the slightest touch. No specimens of diseased orange-stems were received for examination. A microscopic examination shows why the deposit was more easily removed from the orange than the olive leaves. The smooth surface of the former gives no permanent attachment to the fungus, which, as we have before said, does not penetrate into the interior of the cells of the mother plant; while, on the other hand, the hyphæ wind themselves tightly around the stalks of the stellate hairs of the olive, from which they cannot be removed. If the fungus should attack both oranges and olives, it is very evident why the latter would suffer much more than the former. Apart from the absence of hairs, which invariably constitute a large proportion of the scrapings of the olive leaves, that from the orange leaves is precisely identical,—the same moniliform hyphæ, bearing Macrosporium and Helminthosporium spore-like bodies, the same pycnidia and stylospores. Micrometric measurements only confirm the identity. On the orange leaves sent me, there is a greater proportion of pycnidia, and a smaller proportion of stylospores, than in case of the olive leaves; but that is of course an accidental difference, as the olive leaves themselves vary. On the orange, the proportion of Helminthosporium-like spores is much greater than on the olive; but from the facility with which the so-called secondary forms of fruit are produced in fungi, and their great variability, that is not a fact of any importance; and we can in the most decided manner affirm that the fungus is the same on both plants.

The first account of a fungus growing upon orange trees, resembling in its habits that received from California, was given by Persoon, in his 'Mycologia Europæa,' p. 10, published in 1822. His description of the new fungus is very briefly given in the following words: "*Fumago Citri*, late effusa crassiuscula nigro-grisea. Provenit in Europa meridionali ad folia Citri Medicæ, quæ sæpe tota induit." Later, Turpin published an account, with a figure, of a species which he also called *Fumago Citri*, which Montagne made the type of a new genus, Capnodium, published in the 'Annales des Sciences Naturelles,' 3 série, tome 11, 1840. Montagne seems to have had doubts as to the identity of the *Fumago Citri* of Persoon with that of Turpin. Almost simultaneously with

the publication by Montagne of his genus Capnodium, Berkeley and Desmazières published, in the 'Journal of the Horticultural Society of London,' vol. iv. p. 252, an article "On some Moulds referred by Authors to Fumago." In this communication there is the following description of the orange fungus briefly referred to by Persoon and Montagne: "*Capnodium Citri*, Berk. and Desm. Sparsum, setosum; peridiis elongatis; mycelio ramoso moniliformi pulcherrime reticulato; sporidiis oblongis minutis. *Fumago Citri*, Pers., 'Myc. Eur.' vol. i. p. 10; Turpin, l. c. On leaves of different species of Citrus. France: Persoon, Léveillé."

Of fungi occurring on olive trees we have an early account by Montagne, in the 'Annales des Sciences Naturelles,' 3 série, tome 12, 1849, of a fungus mentioned in the 'Bull. Soc. Centr. d'Agric.' 2 série, iv. p. 267, under the name of *Antennaria elæophila*, which had been found at Perpignan in 1829, which caused ravages somewhat the same as the California fungus, and which had previously been referred by him to *Cladosporium Fumago*. It was probably the same plant as the *Torula Oleæ* of Castagne. Tulasne, however, in the 'Carpologia Fungorum,' vol. ii. p. 279, showed that the Freiesian genus Antennaria was the pycnidial state of species of fungi of which Capnodium was the ascigerous state. He restored the old name, Fumago, and gave a detailed account of *Fumago salicina*, which was illustrated in his unrivalled manner.

The fungus from California is evidently the same as that which has been known in Europe since 1829. We have examined two authentic specimens of *Antennaria elæophila*, Mont.—one from the Duby Herbarium, the other from that of De Notaris—and the structure is precisely that of the pycnidial-bearing portion of the California fungus. The stylospore-bearing portion of our fungus is the *Capnodium Citri* of Berkeley and Desmazières, to which they refer the *Fumago Citri* of Persoon and Turpin. Montagne had observed only the pycnidial form—his *Antennaria elæophila*—on olives; whereas, on the orange, he found only the stylospore form, his *Capnodium Citri*. Berkeley and Desmazières make mention only of stylospores on species of Citrus. We have been so fortunate as to find, on the specimens from California, both pycnidia and stylospores, and on both olives and oranges,—which proves the identity of *Antennaria elæophila* (Mont.) and *Capnodium Citri* (Berk. and Desm.). The perfect ascigerous state of the fungus we have not found; nor do Berkeley and Desmazières seem to have met with it, for they add to their description "asci have not been observed." We have not been able to find any recorded instance of asci having been found in *Capnodium Citri*. Tulasne remarks, —quite pertinently, as it seems to us—that, until better known, *Capnodium Citri* and *Antennaria elæophila* can scarcely be con-

sidered distinct from *Fumago salicina.** The specimens from California certainly seem to strengthen Tulasne's suspicions; and we must confess ourselves quite unable to distinguish between *Fumago salicina*—found on willows, oaks, birches, hawthorn, quince, and pear—and *Capnodium Citri*, found on oranges, and, as the Californian specimens show, also on olives. If it be said that no asci have been seen by us, that is no reason why the fungus should be removed from *Fumago salicina*, which, in the conformation of its mycelium, its conidia, pycnidia, and stylospores, it most closely resembles. Evidently, in the group of fungi which we are considering, too much stress must not be laid on the length and shape of the stylospores. We see, in the specimens before us, how great is the variation in what is undoubtedly a single species. Neither is the fact of the branching of the stylospores very significant, as, in the present case, there are both simple and branching stylospores. If the reader will compare our Plate CL., Fig. 1, with that of *Fumago salicina*, by Tulasne, 'Carp. Fung.' plate xxxiv. figs. 14 and 20—leaving out of sight, as far as possible, the different artistic merits of the two,—we think he will admit that in all essential particulars they are alike. In reality the resemblance is even greater than the limited size of our drawing would indicate. We have said that we found no asci; but Plate CL., Fig. 1, *c*, would seem to be the early stage figured by Tulasne, l. c. fig. 20. The asci will probably be found in California; and we do not doubt that they and their contained spores will prove to be like those of *Fumago salicina.*

If we seem to the reader to have gone too minutely into the consideration of the systematic position of the fungus, it was for the purpose of bringing out more forcibly the fact that it is nothing new, or peculiar to California; and that it is not even limited to orange, lemon, and olive trees, but, as we have seen, is found on a number of other trees. How does it happen, then, that a fungus so widely diffused should suddenly increase to such an extent as to injure two important crops? We remarked, in passing, that the hyphæ seemed to be, as it were, gummed to the stellate hairs, and in some cases to one another, by a sticky substance. We do not forget that, when any mycelium is growing on a leaf, a certain amount of dirt—including of course some oily matter—is sure to be entangled in its meshes. In the case of the present fungus, however, there is something more than an accidental accretion of such substances. The surface of the leaves and stems is in many places covered with a gummy deposit, presumably of insect, cer-

* "Donec melius cognoscantur, a Fumagine salicicola supra descripta ægre etiam discriminantur, nisi sede sibi singulis assueta, tum *Fumago Citri*, Persoonio seu *Capnodium Citri* Montanio; tum etiam *Antennaria elæphila*, Montanio," &c. —'Selecta Fungorum Carpologia,' pp. 283, 284.

tainly not of fungus, origin. On this gum the fungus grows luxuriantly; and although it may be found on those parts of the leaves where no gum can be seen, yet it is evident that it has reached such places by growing from the gummy spots. Of the origin of the gum, other than that it does not come from the fungus, we have no theory of our own to advance. Remains of insects are abundant on the leaves; but, being entirely ignorant of entomology, we cannot say what their relation is to the diseased trees. It may be that they are stray visitors caught in the gum. The fungus grows most luxuriantly on the remains of insects which I have seen, which in some cases present a ludicrous spectacle, the hyphæ projecting from them like the quills of a hedgehog.

It has often been asserted by botanists that fungi, of the group to which ours belong, are particularly inclined to attack trees which have been previously infested with insects. In 1849, Berkeley, in the London Journal of Horticulture, described a fungus occurring in Ceylon on coffee—*Triposporium Gardneri*—which followed the appearance of a species of coccus which was described in the same journal by Mr. George Gardner. In their paper on moulds referred to Fumago, Berkeley and Desmazières make the following statement: "They are often, if not always, preceded by honey-dew, whether arising from aphides or from a sugary excretion from the leaves themselves. Frequently, too, they are accompanied by some species of coccus, especially in the genus Citrus." Tulasne * does not agree with the writers just mentioned, as will be seen by the reference. He begins his description of *Fumago salicina*, however, with the following words: "Initio fungillus e membranula constat tenuissima, alba, et hyalina, matricique vivæ instar gummi soluti illitus hæret, quamvis ab eadem, maxime si fortuito ea aruerit, frustulatim aliquando secedat. Id cuticulæ struunt utriculi, perexigui, . . . oleo pallido tandem repleti," &c. This initial stage described by Tulasne is figured in Table xxxiv., fig. 2, mm., l. c. We must confess that the expression "matricique vivæ instar gummi soluti illitus hæret," seems a little indefinite, but the figure looks exceedingly like a collection of oil-globules, or very small eggs. We do not

* " Quibusdam observatoribus visum est *Fumagines* in fructicibus potissimum provenire quos aphides primum occupassent, tamquam si ex humore dulci quem bestiolæ istæ emittunt, aut ex latice viscido quem matrix ab iis læsa copiosum aliquando stillat, suum pabulum traherent; necessitates autem hujus modi duplici de causa minime verisimiles censemus. Hinc enim sexcenties nobis contigit *Fumagines* luxuriantes videre in arboribus, omnis aphidum generis prorsus expertibus; illinc *Fumagines* vere parasitari constat, succis scilicet alienis uti ex his vivis. Super hoc argumento conferas tamen quæ attulit Berkelæus in tomo iv. (1849) Ephemeridis Soc. Hortic. Londinensium, nec non Georgio Gardner commentatiunculam ibidem (pp. 1–6) editam circa the Coffee-bug and Coffee-mildew."—'Carp. Fung.' ii. p. 280.

pretend to say that what Tulasne saw was not a membrane of vegetable substance—a part of the fungus itself; but in the Californian specimens we had something which looked very much like the mm. of Tulasne's figure, and in this case we have satisfied ourselves, by observation and experiment, that it is of animal nature, and not a part of the fungus, which, instead, was growing upon it. It is a little difficult to understand, from what is already known of the development of fungi, how any fungus could begin as a very thin membrane, composed of small cells filled with oil. The initial stage of fungi, if we except the Myxomycetes, as far as we know, is filamentous, not membranous.

The result of our examination of the diseased orange and olive leaves is briefly as follows: The disease, although first attracting the eye by the presence of a black fungus, is not caused by it, but rather by the attack of some insect, which itself deposits some gummy substance on the leaves and bark, or so wounds the tree as to cause some sticky exudation, on which the fungus especially thrives. It is not denied that the growth of the fungus greatly aggravates the trouble already existing, by so encasing the leaves as to prevent the action of the sunlight; we only say that, in seeking a remedy, we are to look farther back than the fungus itself—to the insect, or whatever it may be, which has made the luxuriant growth of the fungus possible. With regard to the fungus, we are able to assert that it is the same on both olives and oranges—the species described by Berkeley and Desmazières under the name of *Capnodium Citri*, which seems to us, together with the pycnidial state described by Montagne under the name of *Antennaria elæophila*, to be but two states of a species identical with that described by Tulasne as *Fumago salicina*. It remains yet to find the asci on olives or oranges, which will probably be accomplished without difficulty in California. The earliest stages of the fungus should be studied by some one living near orange groves; for, although the disease has been known to attack greenhouse plants, it is not very common or, in that case, so favourable for study. Especially is it to be desired that careful notes of the extent and manner of appearance of the disease, and the climatic and hygrometric conditions attending it, should be carefully recorded.

As a remedy, alkaline soaps, as strong as the trees will bear, will no doubt prove advantageous in case of the oranges; but in the case of the olives, much less good is to be expected, owing to the presence of the stellate hairs on leaves and twigs. With this, our notice of the disease from a botanical stand-point ends; and we commend the subject to the attention of entomologists.—*Bulletin of the Bussy Institution.*

III.—*The Potato Fungus. Germination of the Resting Spores.*

By Worthington G. Smith, F.L.S.

Plates CLI., CLII., CLIII., and CLIV.

Before describing the germination of the resting spores of the fungus which causes the Potato disease, it will be well to briefly state how these resting spores were obtained, and how preserved alive in a state of hybernation for so long a period as a whole year. Readers need not be reminded that I last July obtained the oospores or resting spores by keeping Potato leaves and tubers continually moist. For many years past moisture has been well known to be capable of greatly exciting the growth of *Peronospora infestans*, and De Bary in his recent essay classes the Potato fungus * with "other water fungi." Mr. C. Edmund Broome, of Batheaston, who is known as one of the first cryptogamic botanists of this country, repeated my experiments in the following manner: He selected Potato leaves badly infected with Peronospora, partly crushed them, and placed them in a saucer of water underneath a bell-glass. The saucer was kept in a sloping position, so that the leaves (being partly submerged) were allowed to absorb the water naturally. The result was that he obtained an enormous number of resting spores in all parts of the leaves, many being within the spiral vessels and hairs. These resting spores were in every way identical with mine, and they could only belong to the Peronosporeæ or Saprolegnieæ, because similar bodies are unknown in other families of fungi. The first-named family has jointed threads, the second bears threads without joints; now as the threads seen by me, and last year illustrated in connection with the resting spores, had jointed threads, they must belong to Peronospora, and not to Saprolegnia. As there is no other Peronospora than *P. infestans* known to grow upon the Potato plant, it is clear that the resting spores cannot rationally be referred to any other than the Potato fungus. Added to this, I last year saw the secondary bodies clearly growing from the Peronospora threads. I attach great importance to the jointed threads, because De Bary, when he figures Artotrogus from "Montagne's original specimen," † shows the threads with many septa. From the first I have said that Montagne's Artotrogus and the bodies discovered by me are the same. That both belong to Peronospora the sequel will prove.

It was of the highest importance that these resting spores should be preserved alive till the time arrived for their renewed activity, and with this purpose in view I preserved the material in

* 'Researches,' p. 242. † Ibid. p. 258.

which the resting spores were present in sealed bottles, each bottle containing more or less pure water or expressed juice of horse-dung diluted with water. As I was quite in the dark as to the habits of these resting spores, of course I did not know what to do for the best, or what the result of my experiments would be. In former numbers of the 'Gardeners' Chronicle' I have described how these resting spores at first floated on the surface of the water, how they at length deposited themselves in the sediment at the bottom, and how on opening one of the bottles at the last meeting of botanists at Hereford the resting spores were found still intact and apparently alive. Happily, nearly all my spores retained their vitality. Mr. Broome, being equally uncertain with myself, trusted to chance, and chance so far favoured him that all his resting spores in the slanting saucer of water well retained their life. It might have been (and even was) said that possibly some fungus foreign to the Potato fungus had got into my material, but if so it must be regarded as a coincidence in the highest degree extraordinary that Mr. Broome should also get the same new and foreign fungus in his Peronospora material—a body so puzzling in its nature as to be referred to no less than eight different species of fungi.

All who have studied the habits of the lower fungi know the extreme difficulty of preserving the specimens alive. This difficulty almost amounts to an impossibility. The fungi under study may be present one day, and all gone the next; a few drops of extra moisture or a slight current of dry air is sufficient to destroy or collapse the whole lot. Besides this, myriads of other parasitic fungi and whole tribes of infusoria commonly make their appearance and prey upon the material that is desired to be preserved.

Now one of the most extraordinary facts about the recent Potato investigations in this country is this. These other fungi and infusoria have not to any damaging extent appeared. Since I opened my sealed bottles last April I have kept the material under a bell-glass, and there has been no offensive odour, and to no appreciable extent have there been any moulds, infusoria, or parasites, except *Peronospora infestans* itself, and the other fungus which is equally destructive to potatoes, known under *Fusisporium Solani*. In investigating the Potato disease it was almost as important to discover the entire life history of the Fusisporium as the Peronospora, and fortunately the materials preserved gave a perfect clue to the entire life history of both. Mr. Broome's material has in the same manner been free from an excessive number of other fungi and infusoria.

The germination of the resting spores was awaited with the greatest anxiety; and as I never knew from one day to another whether or not these bodies might all collapse and perish, I was

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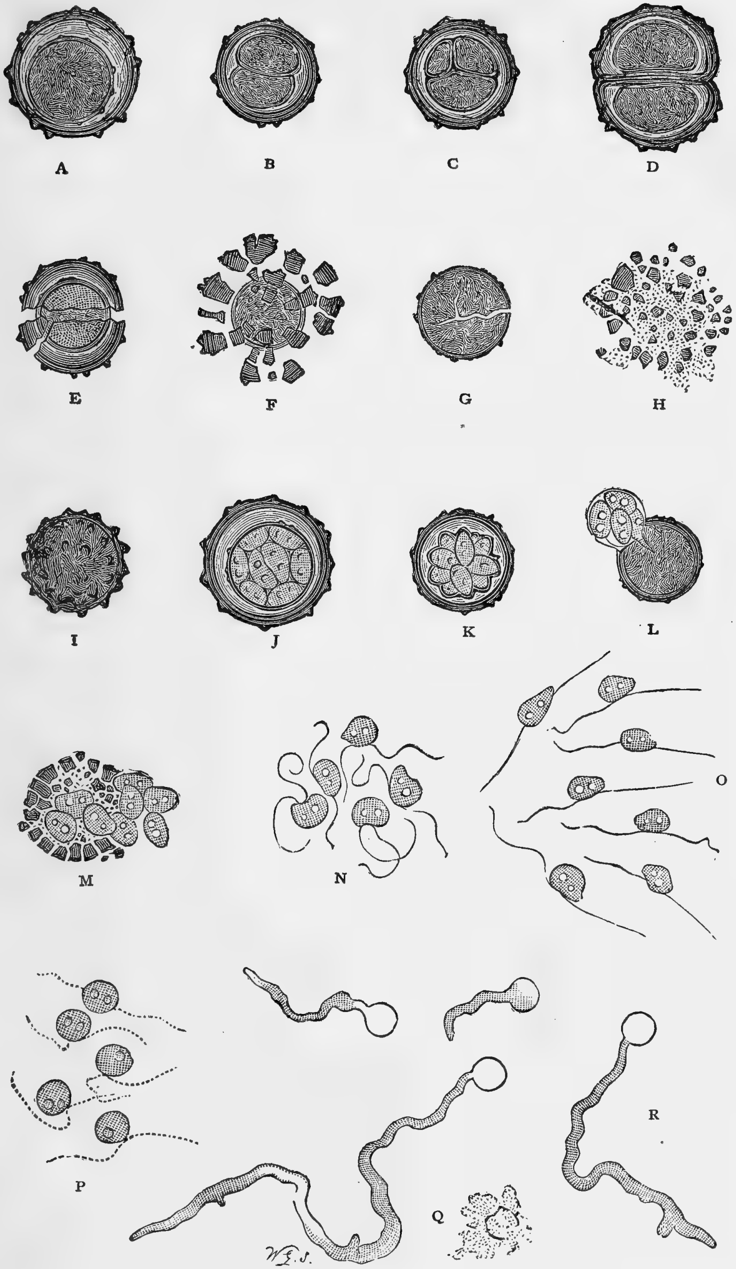
under the necessity of dividing the material, and keeping a constant look-out for results under different conditions. With this object in view, therefore, I kept some of the bodies moist in pure water, others in diluted expressed juice of horse-dung, others in expressed juice of fresh Potato leaves, others upon extremely thin slices of Potato and on crushed Potato mash, others in saccharine fluid, others in nitrogen gas, some between pieces of glass kept constantly moist, some upon broken tile (also kept constantly moist), and some upon Potato leaves as they grew upon the living plant. Besides this, I have had a quarter of a hundred of slices, kept damp, and under examination every day (almost night and day) for the last three months. All these preparations I have kept constantly and uniformly moist under darkened bell-glasses, for darkness invariably assists the growth of spores of all sorts.

The first new fact worthy of note is this: many of the resting spores grew in size during nine months of their rest to twice their original diameter, or about four times their original bulk, and their aspect gradually changed from almost smooth, semi-transparent bladders to brown, more or less rough and warted or echinulate spheres. These latter brown, mature bodies were quite the same in character with those so sparingly seen last June and July. How they arose last year no one saw, but probably the wet weather of the early summer caused their appearance. It does not follow, because the resting spores have taken a year to artificially mature with me, that therefore they always take a year to ripen; it is quite possible that, in a state of nature and under different conditions, they may mature rapidly. At any rate, two sorts of bodies were seen together last year, transparent smooth bodies, and rough brown ones. I considered them to be different states of the same resting spores, and subsequent facts have proved my supposition to be quite correct.

The top row of illustrations on Plate CLI. shows characteristic conditions of the almost mature reproductive bodies as drawn in April last. At A is seen the oospore (or resting spore) within the oogonium (bladder which holds the resting spore), at B may be seen two resting spores within one oogonium, and at C three resting spores within one oogonium, whilst at D is shown a double oogonium—two oogonia coalesced, and each oogonium containing a resting spore.

At the end of April and beginning of May last I began to see the first signs of germination, and at this time many of the oospores proved effete; the oogonium cracked at E, or became broken into atoms, as at F, discharging a bladder, as at G, which perished in fine dust, as at H.

As the month of May progressed many of the resting spores



The Resting Spores of the Potato Fungus. Enlarged 400 diameters.

became dense and dark, with the oospore occupying the whole of the oogonium as at I; this condition is different from that of the body A, for in this the resting spore, being not quite mature, does not yet occupy all the oogonium, but floats within from side to side, as the object happens to be moved under the microscope. J shows the contents of oospore being broken up into zoospores; K shows the zoospores within still more clearly, and where they are giving an echinulate appearance to the bladder within (an appearance adverted to lately by Mr. Berkeley in a letter to the 'Gardeners' Chronicle'); L shows the bladder from within the oospore being discharged from the oogonium after the manner of Cystopus, with the contained zoospores; this bladder frequently breaks up into dust, as at M, setting the zoospores which are at present quiescent free, as at N; two tails shortly appear on these latter bodies, and at a certain period of their growth the anterior cilium, or tail, is pushed straight out as seen at O, the posterior tail then quivers with an undulatory movement, and the zoospores sail out of the field of the microscope. How long the zoospores live it is difficult to say, but probably somewhere between twelve hours and a week; at length they come to rest, as at P, when the tails fall into fine dust. Some zoospores burst and at once perish, as at Q, whilst others throw out threads of mycelium, R, which threads are destined at length to bear the conidiophores of the Potato fungus in its new generation. The zoospores thus obtained were planted on the foliage, and upon thin slices of Potato supplied from a frame by Mr. Alfred Smee. On these materials they at once produced mycelium and small conidiophores, which, without doubt, belonged to Peronospora, but as better results were afterwards obtained from resting spores similar to I, Plate CLI., the figures are not here engraved.

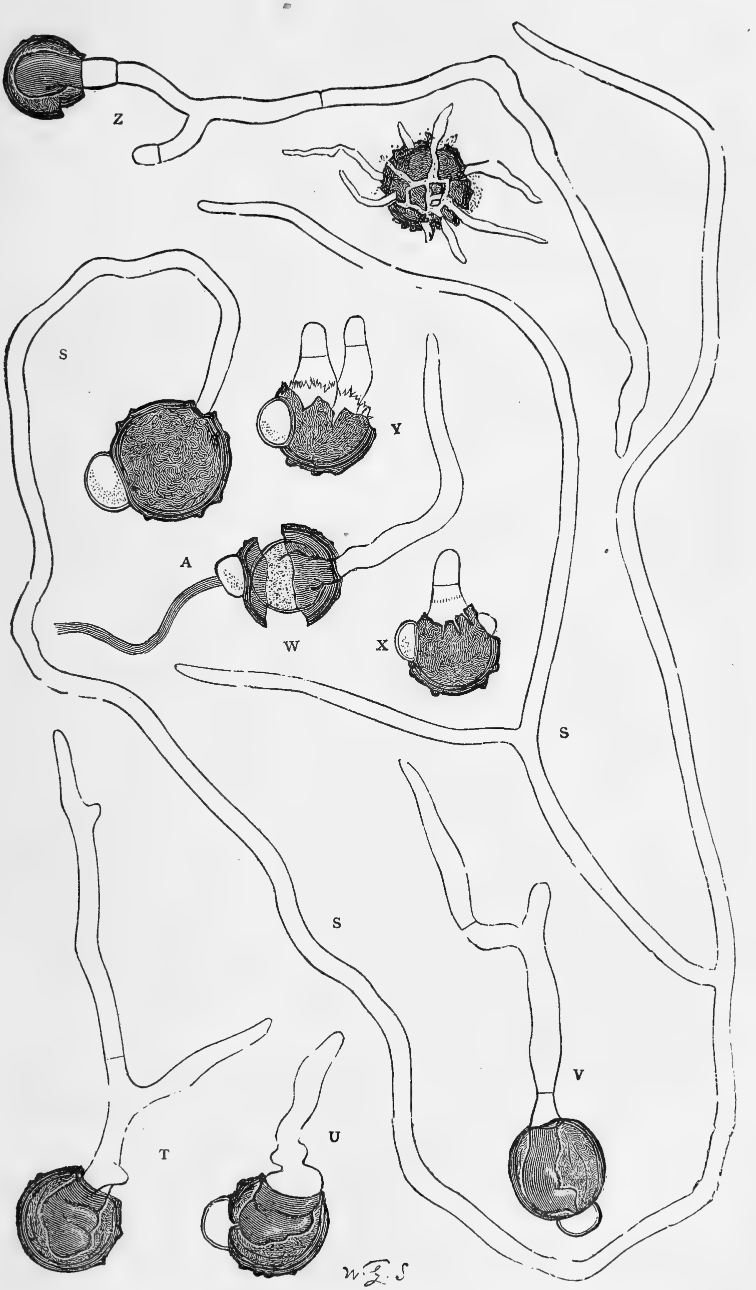
The Rev. J. E. Vize, Forden Vicarage, Welshpool, a gentleman who has made a special study of microscopic fungi, has had some of my living material under examination during the past winter and spring, and when the first signs of germination showed themselves in my oospores, I wrote him to keep a good look-out for results. He wrote me as follows, under date of April 21: "My idea certainly is that the oospores are germinating: bottle No. 1 had a thin film on it which developed into a lot of mycelium and threads of Peronospora;" I too observed the same fact in London.

Throughout May the habit of the oospores appeared to remarkably change, for instead of producing zoospores they protruded a thick and generally jointed thread, this thread agreeing exactly in size with average *Peronospora infestans* threads. On May 13 I observed on the preparations treated with expressed juice of horse-

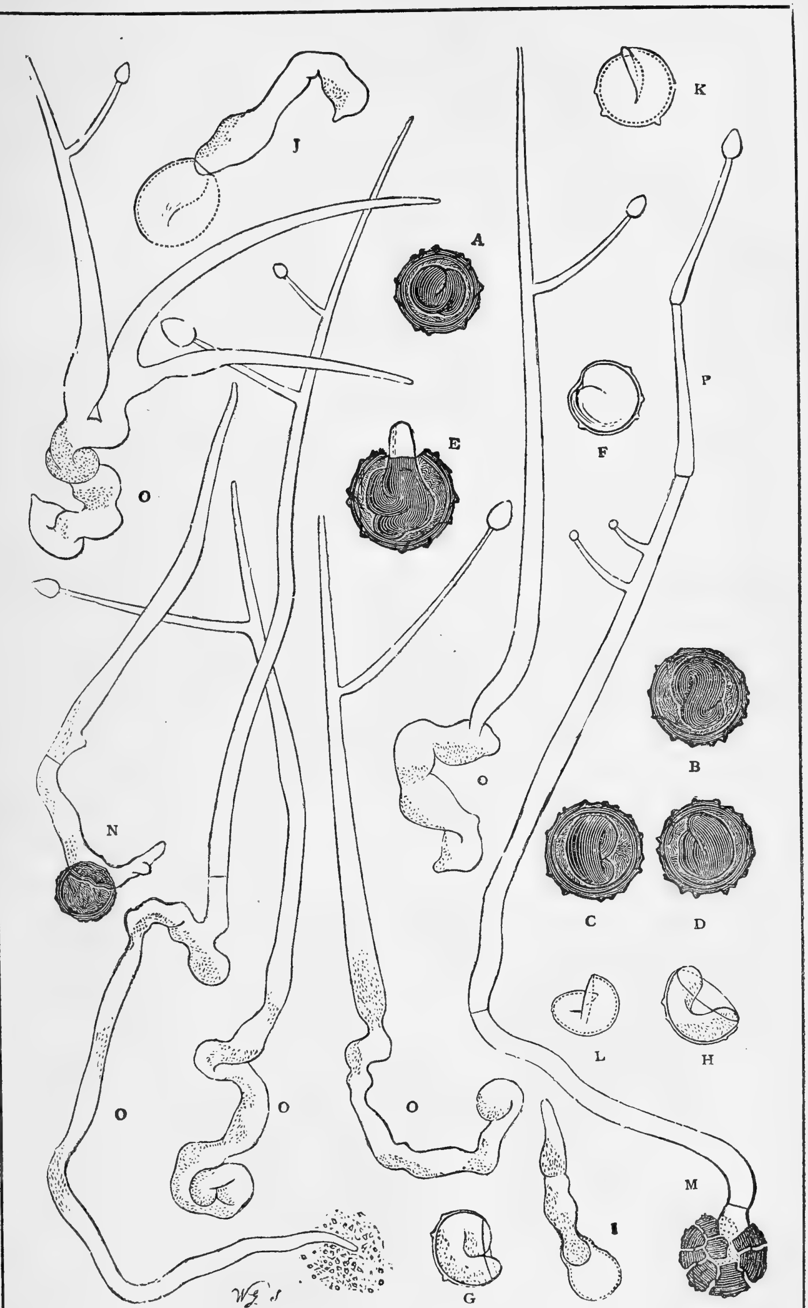
dung threads similar to the very long branched thread shown at S, S, S, Plate CLII.; these threads were so long that they traversed the entire slide, and I could only detect a single septum or joint, and frequently none. T, U, V, are characteristic: the latter shows two septa, which is a common condition at this stage of growth; and all three figures show the protoplasm of the oospore coiled up within the walls of the latter. W shows an oospore germinating with the antheridium (A) attached to the oogonium, and still upon its last year's thread; X is a germinating oospore with a thread showing the first septum; and Y shows two germinating oospores emerging from one oogonium, each thread showing the first septum; the old male organ (antheridium) is still attached to W, X, and Y. The figure at Z, drawn on May 12, is characteristic, and shows three septa; the specimen was sent on to the Rev. M. J. Berkeley, who replied: "I found the germinating oospore exactly as you figure it. There can be no doubt about the matter." Mr. Broome, who was examining similar material of his own, wrote on May 4: "It only remains now apparently to see the Peronospora arising from the threads which proceed from the oogonia to prove the identity;" and again on May 20: "I do not see any attached conidia, but the space between the sections of Potato is covered with long threads resembling the conidiophorous threads, but I could not see any with the spores on them." It may be said here that no other known fungus has conidiophorous threads similar to those of the Potato fungus.

At the beginning of May, whilst observing the habit of Fusisporium and its resting state, I observed typical *Peronospora infestans* growing upon the drier parts of the previous year's crushed and decayed leaves; this observation was confirmed by Mr. Vize, who wrote on May 22: "According to my examination the Peronospora grows on the drier parts of the magma. I do not observe it growing on the very wet."

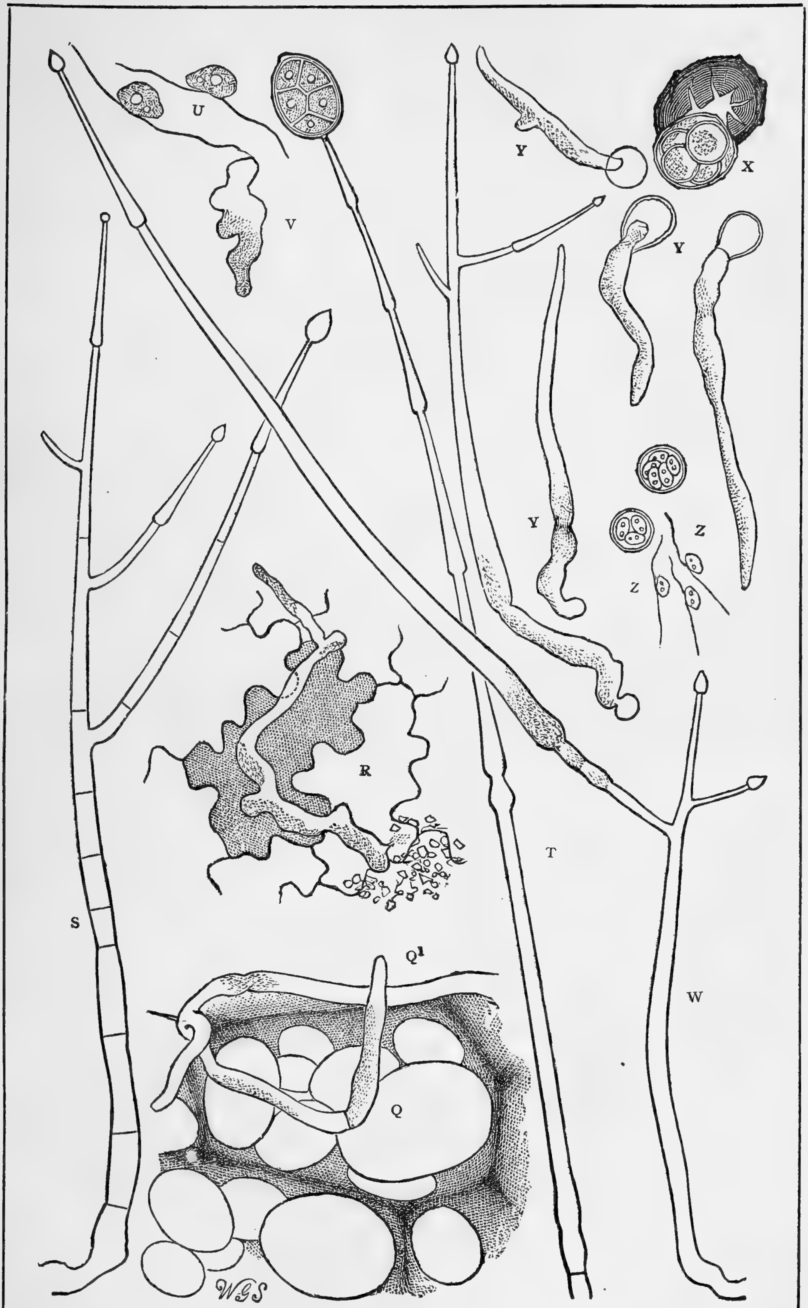
On Plates CLIII. and CLIV. may be seen a collection of resting spores before and in the act of germination, together with a number of Peronospora threads taken from Potato leaves and tubers previously infected with the oospores. A, B, C, and D show oospores in which the protoplasm which is destined to produce the new plant is coiled up within. At E this coil is seen just emerging. This convolute mass is really contained within a thin bladder, and sometimes the bladder is expelled, as in Cystopus, from the oogonium before the coil unwinds, as at F, G. The thread then emerges as shown at H, I, and J, sometimes leaving the bladder free but broken, as at K, L. It is rare to see the thread of the new plant in connection with the oogonium, as at M, N, though I have so seen it, together with the septa many times. The first mycelium or spawn of the new plant is seen at O, O, O, and from this the



The Resting Spores of the Potato Fungus. Enlarged 400 diameters.



The Resting Spores of the Potato Fungus. Enlarged 400 diameters.



The Resting Spores of the Potato Fungus. Enlarged 400 diameters.

Peronospora springs direct, and (when artificially grown) almost invariably in a terminal manner. The conidia are not mature in any of the specimens here figured; doubtless this is because all the plants are more or less abnormal from being grown artificially, but still the threads are characteristic of *Peronospora infestans*, and no known fungus but the one which causes the Potato disease has vesicular swellings such as are shown at P.

Mr. Chas. B. Plowright (surgeon, of King's Lynn, a gentleman who has long studied fungi) has patiently examined some of the living material with which I have been working this spring and early summer, and he writes me on May 19 to say: "I find plenty of branching, nodose conidiophores, especially amongst the drier portions of the substance sent. I also see living conidia. I have seen many conidiophores with convoluted bases, but in the vast majority of cases long ere the conidia come the oospore is gone; I see the granular protoplasm distinctly ascending the base of the conidiophore." As regards the first coil of mycelium, Mr. Plowright writes: "I distinctly saw this curved in two oospores, and I believe the mycelium comes out with a curl." The same gentleman, under date May 19, writes: "I saw a great many conidiophores both with conidia *in situ* and not; most conidia had fallen off; latterly I saw plenty of convoluted bases." The evidence of identity appears complete, and many of the figures here published, and others not published, have been confirmed by Messrs. Vize and Plowright.

At Q, Plate CLIV., may be seen Peronospora mycelium with a young plant (Q^1) growing from amongst the starch of the Potato tuber, the dark background showing the cell-wall corroded by the fungus, and at R a similar fragment of mycelium upon the cuticle of a Potato leaf; it is very common to see one cell of the cuticle thus discoloured by the corrosive mycelium, the corrosion of the cell being caused by the mycelium passing over and upon it. Both threads here shown come direct from last year's resting spores. At S is engraved a branch of the Potato fungus, showing the numerous partitions with which the threads are at times furnished, and at T is a typical well-grown branch of the fungus, with a full-grown conidium at the apex; this conidium may either discharge zoospores, as at U, or an irregular mass of protoplasm, as at V, from either of which a new plant may spring, and in this habit the conidium agrees well with the resting spore; the branch in this figure is shown as continuous, and though furnished with the vesicular swellings no partitions are present, the branches are frequently so seen. At W is illustrated a small weak plant, giving rise to a branch, which branch is developing into a large and strong plant; such a phenomenon is by no means uncommon, and shows how the fungus may at times be proliferous and how it increases itself in

every possible way. I have frequently seen similar secondary threads branched.

During the last hours of completing the last two Plates illustrative of the Potato fungus, a new and curious fact came to light. On examining the oospores in saccharine fluid, I observed some of the discharged bladders to be carrying from two to four secondary bladders inside (X); these secondary bodies were in their turn expelled, and grew and produced mycelium as at Y, Y, Y, whilst a few of the same secondary bladders burst and produced from three to six very small zoospores, generally only three. It is a most singular fact that these secondary bladders and zoospores are exactly the same in size with De Bary's *Pythium vexans*, and about one-sixth or eighth of the bulk of the resting spores from which they were discharged. With this exception there has not been the slightest approach in any of my material to organisms which might be referred to Pythium. Mr. Plowright writes: "None of my oospores ever burst and produced Pythium or Pythium-like spores."

My material has contained a large number of dead mites and aphides and a few nematoid worms; the oogonia and threads were to be seen in all parts of the dead insects, but not in the worms.

De Bary, in reviewing my observations, says: "Even if the often-mentioned warty bodies were hibernating oospores of Phytophthora (Peronospora), like the similar oospores of *P. Arenariæ* which resemble them, we should not gain much information bearing upon these questions, since their occurrence is, at the best, extraordinarily rare." This sentence is very erroneous, for although the bodies were *apparently* rare when I first recorded their discovery, they were not necessarily so in a state of nature, for on continuing the experiments after my first essay was written, the resting spores were produced in myriads, and that too within the tissues of a comparatively few leaves. During the present spring I have sent mounted preparations of the mature (or almost mature) resting spores to many of the foremost cryptogamic botanists of Europe, but not one has denied their possible identity with *Peronospora infestans.*

For more than thirty years our Potato crops have been systematically destroyed by two virulent fungi, viz. *Peronospora infestans* and *Fusisporium Solani;* these two parasites almost invariably work in company with each other; they suddenly appear for a few weeks, destroy our crops, and vanish for ten or twelve months, then reappear and repeat the work of destruction. I claim for my work that it is new, and that it has proved how both these fungi hide and sleep through eleven months of the year. As I have kept the resting spores of both parasites alive artificially in decayed Potato leaves in water, in moist air, and in

expressed diluted juice of horse-dung, it conclusively proves to me that the resting spores hibernate naturally in the same manner. The seat of danger from both parasites is clearly in dungheaps, ditch sides, and decaying Potato plants.

Any method of destroying the resting spores of these pests, or of warding off or mitigating their attacks, obviously depends in a great measure upon a full knowledge of their life history. That life history I have endeavoured to the best of my ability to watch and describe, and I am content to let the observations stand on their own merits. Sensibly conducted and extensive field experiments might probably teach some valuable lessons; but it is difficult, if not impossible, for any single individual, whether farmer or botanist, to institute and carry out such experiments.—*Gardeners' Chronicle*, pp. 39-42, 1876.

IV.—*The Affinity of the Mollusca and Molluscoida.*

By W. K. Brooks, Ph.D.

During last August and September (1875) I enjoyed, through the kindness of M. Agassiz, an opportunity of studying the development of several of our more common marine Gasteropoda; and the results reached seem to point to the conclusion, which I believe has never been pointed out, that although the Gasteropoda are much more specialized and highly evolved than the Lamellibranchs, nearly all their organs, excepting those of locomotion and relation, conform much more closely to the embryonic type than do the same organs in an adult Lamellibranch. The latter group must therefore be regarded as a side branch from the main stem, of which the Gasteropoda are a much more direct continuation.

I have already shown * that the embryonic shell of Anodonta is, at first, a cup covering what is to become the dorsal surface of the embryo, and is therefore homologous with the shell of a Gasteropod. This cup or hood soon folds down on to the sides of the embryo, precisely as described in Dentalium by Lacaze-Duthiers, and at a very early period splits along the dorsal median line and becomes separated into the two halves of a bivalve shell, which are thus shown to be together the homologue of the shell of a Gasteropod exclusive of the operculum, which, as Selenka has shown in his 'Entwickelung von Tergipes claviger,' is formed by a split which extends across the long axis of the body, and therefore at right angles to that which, in Anodonta, gives rise to the two valves. The valves of an adult lamellibranchiate shell are a specialization

* 'Proc. Amer. Association,' 1875.

of the embryonic shell; are bilateral in origin, and together represent the dorsal or hæmal cup or shell of a Gasteropod, a Polyzoon, or a Brachiopod; while the ventral or neural operculum of a Gasteropod corresponds to the neural valve of a Brachiopod or the lid of a cheilostomatous Polyzoon, and is wanting in the Lamellibranchs.

The digestive organs of an adult Lamellibranch, although they are very much less specialized than those of a Gasteropod, seem to be much more widely removed from the embryonic type. The stomach of the Veliger of Astyris, like that of a Polyzoon, is divided by a constriction into two chambers. (Compare also the figure of the embryo of the Pteropod, *Carolinia tridentata* by H. Fol, and that of Limnæa by Rabl.) In the embryo of Mytilus we have, according to Lacaze-Duthiers, a similar stomach, and in the adult of Yoldia we have the same a little modified; here the anterior portion of the stomach receives the bile-tubes; and the posterior portion is prolonged so as to form a conical, somewhat twisted, intestine-like pouch, from the bottom of which the small intestine originates. In Venus this peculiarity is much more marked; the posterior chamber is now tubular, and sharply separated from the true stomach, which represents the anterior half of the embryonic stomach. The tube is somewhat convoluted, and is imperfectly divided by a longitudinal fold of the inner wall into two parallel chambers, of which the anterior is the true intestinal cavity, while the posterior contains the crystalline style. In Cardium we find the process of differentiation carried a step farther. The partition, which in Venus is imperfect, here extends entirely across the tube, so that the cavity of the sheath of the style is completely shut off from that of the large intestine, although the two are still in contact, and are contained within the same outer wall. Solen will answer as an illustration of the next step in the process of differentiation. Here the large intestine is not united to the sheath of the style, although the former is nearly straight, and parallel to as well as near the latter. In such forms as Mya the large intestine is entirely independent of the sheath of the style, and its large semicircular convolutions begin at the point where it joins the stomach. This series seems to show that the stomach of a Lamellibranch is homologous with only the anterior half of that of the embryo, or of a Gasteropod, while the large intestine and sheath of the style are together a very peculiar modification of the posterior portion.

In the prosobranchiate Gasteropoda, as in the Lamellibranchs, the gill is formed as a series of tentacular prolongations into the mantle chamber; these increase in number, and at last form a broad sheet, which is well shown beneath the transparent shell of Crepidula during the later "Veliger" and the early "Gasteropod"

stages. In the Gasteropoda these tentacles remain free from each other during the whole life, and the water circulates over and around them; while in the Lamellibranchs they become so bent upon themselves and united to each other, that the gill-tubes are formed, and the water is driven into and through these, to be discharged into the cloaca, which is a special chamber, peculiar to the Lamellibranchs. In such a form as Mytilus, where the union between the tentacles is somewhat imperfect, we have what appears to be an intermediate stage between the perfect lamella of Mya or Unio and the separate tentacles of a Gasteropod. The gills of a Lamellibranch are therefore, like the shell and the digestive organs, a specialized form of the embryonic type, which is pretty closely adhered to in the adult Gasteropod.

These facts must not be regarded as showing that the Lamellibranchs are higher than or derived directly from the Gasteropods, for any such conclusion is rendered impossible by the lack in the latter group of such peculiarities as the lingual ribbon; a centralized and highly evolved nervous system; and accessory organs of reproduction. Although it is true that these features might have been lost through adaptation to a sedentary life, their entire absence at all stages of growth, throughout the whole class, would seem to indicate that they never existed; so we cannot derive these animals directly from the Gasteropoda, but must regard them as an offshoot from a form of which the Gasteropods are the highly developed linear or nearly linear descendants. If this conclusion is accepted, it is plain that all attempts to trace the phylogeny of the higher Mollusca through the Lamellibranchs to the Molluscoida, must be erroneous and useless.

The history of the discussion of the affinities of the Mollusca is an almost unbroken record of generalizations based upon imperfect knowledge and erroneous conceptions, and so many arrangements of the group have been proposed, accepted for a time, and then shown to be unnatural, that it is not at all strange that many naturalists should now call in question the existence of any real affinity between the higher and the lower classes. As long as the attention of the investigator was confined to the study of shells, there seemed to be no difficulty in connecting the Lamellibranchs with the Brachiopods through such forms as Anomia; and although the slightest anatomical knowledge is sufficient to show that the resemblance between these forms is entirely superficial and without scientific value, this conception had been so generally accepted and so firmly established, that the confirmation by embryology of the results reached through anatomical research, has scarcely been able to thoroughly exterminate it.

This view has been replaced by another which is not open to the charge of superficiality, since it is based upon a thorough know-

ledge of adult structure, and its weakness is shown only when it is tested by embryology. The clearest and most forcible statement of this view is that given by Allman in his 'Fresh-water Polyzoa.' According to Allman, the Tunicata are intermediate between the Polyzoa below and the Lamellibranchs above. The branchial sac of a Tunicate represents the permanently retracted tentacular crown of a hippocrepian Polyzoon; the tentacles form the horizontal bars of the sac, and uniting to each other at intervals enclose the branchial slits. Although Allman's figures are necessarily diagrams, no organ is exaggerated or suppressed for the purpose of making the likeness more forcible; they are very accurate and faithful representations of the animals, and show the closest similarity between these two forms; the position, structure, and connections of almost every organ of the one being duplicated in the other. An almost equally perfect comparison may be made between a Tunicate and a Lamellibranch, but the recent great additions to our knowledge of the embryology of the Tunicate seem to show, with absolute conclusiveness, that we here have nothing but a very perfect and striking adult resemblance, reached in each of the groups in a different way, and therefore without homological signification. Whatever view of the Vertebrate affinity of the Tunicate we may incline to, we must recognize the fact that the branchial sac is morphologically part of the digestive tract, and in no sense whatever a lophophore or a tentacular gill. Moreover, we should expect, according to all analogy, to find the affinity to other groups most clearly shown in the low or embryonic forms, but Appendicularia presents none of the peculiarities upon which the comparison is based. As Ray Lankester has lately referred to Allman's homology in a way which seems to imply that he still accepts it, I will repeat more briefly my reasons for rejecting it. These are, first, that the development of the Tunicate shows that the resemblance is not due to community of origin, but is reached in different ways; and secondly, that the adult Lamellibranchs are a specialization of the embryonic type, and therefore cannot lie in the direct line connecting the Molluscoida with the Mollusca. Allman himself seems to have seen the force of the first objection, for in a much later paper (1869) he advances the view that the Polyzoa are connected, through Rhabdopleura, with the Lamellibranchs. His studies of this genus were made upon alcoholic specimens; and Sars, who enjoyed the superior advantages afforded by an abundance of living specimens, has shown that Allman was mistaken in regard to almost every one of the points upon which he attempted to establish the supposed relationship.

These are only a few of the arrangements of the Mollusca which have been proposed, and the fact that, of the three selected, two are by Allman must not be regarded as the result of a wish to un-

favourably criticise the work of this author. On the contrary, the anatomical resemblances which he points out so clearly are worthy of the most thoughtful attention, and although they are not homological and do not indicate descent, they are excellent illustrations of the independent origin of similar structures; a class of relations which has not yet been sufficiently allowed for in the speculations of the modern school of zoology, but which seems destined to form, at some future time, an important element in the theory of the evolution of life. The superiority of the conceptions of Allman becomes evident as soon as we contrast them with many which have been advanced; for example, the comparison advocated by a very distinguished naturalist and embryologist between the foot of a Lamellibranch, the tail of Appendicularia, and the placenta of Salpa.

We come now to the question, If our present knowledge of the embryology of the Mollusca and Molluscoida disproves all the old ideas of their affinity, does it present anything to replace them?

Most of the Gasteropoda are known to pass through a free, locomotive "Veliger" stage. The veligers of different Gasteropods differ considerably in form; and in some the embryo, at this stage, is much less specialized than in others; but, omitting the complications introduced as adaptations to a spiral shell, the veliger of such a marine Gasteropod as Astyris may be regarded as presenting the typical form. A veliger may be described as a free-swimming, bilaterally symmetrical embryo, without a true heart or vascular system, or branchiæ, with the mouth and anus near each other on the median line. The digestive organs are suspended in the body cavity, and attached to the body-wall at the two external apertures, and by the various muscles. The foot is situated between these two openings; and the pedal ganglia, which are in most veligers the first ganglia to appear, are developed in the region of the foot; that is, between the mouth and the anus. The foot is generally supplied with a bunch of setæ, which are apparently sensory in function. The animal is enclosed in a shell composed of two portions; a large ventral cup, and a neural or pedal operculum, which is united to the anal margin of the cup at the earliest stages, and subsequently becomes separated from it. This shell and lid are found in the embryos of those forms where the adult is without an operculum, as Crepidula, as well as in those where the adult is destitute of a shell, as the Nudibranchs.

The most characteristic peculiarity of the veliger is the *velum.* This is a large bilaterally symmetrical circlet of cilia, developed from the cephalic region of the embryo, and supported, at some distance from the body, by a transparent double-walled veil, the cavity of which is irregularly divided into large sinuses, in free communication with the body cavity. The animal swims, usually near the surface of the ocean, by means of the long cilia of the velum, which

would seem to perform the function of a respiratory organ as well, for the fluid which fills the body cavity is driven into and out of the sinuses of the velum by the retraction and expansion of this structure; in most veligers this circulation seems to be aided by the rhythmical contraction of the muscular fibres which bind the foot to the œsophagus. The mouth is not within the circlet of large locomotive cilia, but immediately behind it, and a ring or band of smaller cilia passes from the anterior margin of the mouth entirely around the velum, on its lower surface, and therefore outside the circlet of locomotive cilia. This second circlet seems adapted to convey food to the mouth, but there are no direct observations upon this point. The velum and the foot are retracted into the shell by the action of a pair of long muscles which pass from the sides of the œsophagus and region of the foot to the bottom of the ventral shell, and subsequently become the columellar muscle of the adult.

The veliger stage seems to be represented very perfectly in most of the marine Gasteropods, except some of those whose eggs are protected by strong cases, within which the early stages of development are passed. In some of these, as Purpura, there is a well-marked but somewhat rudimentary veliger stage, and it is probably represented more or less faintly in all, although the embryo does not pass this period in free locomotive life, and accordingly has no need of swimming organs.

Although the marine Opisthobranchs pass through a perfect veliger stage, and are locomotive at this period, the fresh-water Pulmonates undergo their embryonic development within the egg, and with them the velum is only faintly indicated, and it appears to be entirely wanting in the land Pulmonates whose young are not aquatic.

As regards the remaining classes of the Mollusca, the Scaphopods pass through an embryonic form which is easily recognized as a veliger, although it is not very highly developed. It would seem as if the Lamellibranchs, from their fixed or nearly fixed mode of life, had an especial need for a locomotive larval stage, but the veliger stage can hardly be detected in this group. Embryos of several of the marine Lamellibranchs have been described and figured as furnished with a circlet of cilia, and thus fitted for locomotion, but these embryos are so rudimentary in other respects, and so different from the highly specialized veligers of the Gasteropoda, that we cannot, with any safety, say that they represent this stage of development at all, although the fact that Anodonta has an unmistakable velum would seem to indicate that the Lamellibranchs, like the Gasteropods, are the descendants of a free-swimming veliger, and that the circlet of cilia described in the embryos of such forms as Cardium is also to be regarded as a rudiment of the same stage. It may be that the development of the young within the branchiæ

or the mantle chamber in this class does away with the necessity for a locomotive embryo, but at present we know so little of the life history of the marine forms that we have very little ground for generalization. The imperfection of our present knowledge cannot, however, be fairly urged to restrain us from making as much use as possible of what knowledge we do possess, although we must constantly bear in mind that it introduces an element of uncertainty into all of our conclusions. This of course is true of all biological speculation at present, but no one would advocate the abandonment of all speculation and comparison until all of the facts of our science have been recorded and verified.

The embryo of Anodonta, at a very early stage, has, at the anterior end of the worm-like body, a simple band of cilia; as development progresses this is carried, by the formation of the mantle lobes, into the mantle cavity, and there increases in length, and the free ends bend towards each other and finally unite, thus forming a closed, bilaterally lobed circlet like that of the Gasteropods, except that it is not raised from the surface of the body, and its cilia are very short and are not used for locomotion. It is interesting to notice also that it is attached to the dorsal surface of the shell by two muscles like those of the veliger of a Gasteropod. In Anodonta these subsequently become the retractor muscles of the foot.

The thecosomatous Pteropoda present the veliger stage of development in a form as highly specialized as that of the marine Gasteropoda, and the embryos of the two do not differ at this time in any essential particular. The development of the gymnosomatous Pteropods, on the contrary, is entirely anomalous, and at present appears to be inexplicable on any theory of descent.

In the Cephalopoda, as so often happens in the higher representatives of a group, the indirect course of development has given place to the direct; the larval stages are usually entirely wanting, and the embryo shapes itself, from the beginning, into the form of the adult. In most Cephalopods there is no trace of a veliger stage, but its absence is what we should expect from the analogy of the higher forms of other groups.

The conclusion to be drawn from our present knowledge of the Mollusca will appear, from this review, to be that all of them are to be traced back to a free-swimming ancestral form, of which the veliger embryo is the representative; this seems to be the only way in which we can account for its appearance in at least certain representatives of so many widely separated groups; and the presence of rudiments of it in such forms as Anodonta and the Pulmonates seems to indicate the same conclusion. We have seen that in many of the cases where it is wanting, its absence can be reconciled with this theory, even with our present knowledge, and we may therefore hope that a more complete acquaintance with the em-

bryology of the naked Pteropods will show that they are not an exception.

We come now to the interesting question, What are the affinities of this "Veliger" from which the true Mollusca are descended?

It is only necessary to glance at the side view of any fully developed veliger, such as Selenka's figure of Tergipes, in order to notice the resemblance to a Polyzoön, and more careful examination shows that the resemblance holds not only in the general plan, but in detail. The velum corresponds to the lophophore in position and structure, and subserves, like this, the function of respiration, and probably that of ingestion as well. The heart is absent in both, and the fluid which fills the body cavity and bathes the digestive organs is kept in motion by the contraction of the various muscles of the body. The digestive organs are similar in form and also in their connections. The epistome with its ganglion answers to the foot and pedal ganglia, and in Rhabdopleura the epistome is functionally as well as morphologically a creeping disk. The shell and operculum answer to the cell and lid of a cheilostomatous Polyzoon, and the retractor muscles are clearly homologous. The most important differences seem to be that, among the Polyzoa, the animals are fixed and multiply by budding; and that in all, the mouth, as well as the epistome, is within the circlet of the lophophore. (Rhabdopleura was described by Allman as an exception in this respect; Sars, however, has shown that although the tentacle-bearing portion comes to an end upon the sides of the foot, the line of cilia is continued entirely around it.) The lack of agreement between the positions occupied by the mouth and foot in the two forms seems to be the most serious objection which can be urged against the view here advocated. In answer to it we can only point out that in Dentalium the mouth is formed within the circlet, although the foot is outside it. It is not to be supposed, however, that the Veliger can be traced back to any existing form of Polyzoon, or even to any order of this class. In some respects its affinities are with the Hippocrepia, in others they are with the Cheilostomata, and in still others they are with Rhabdopleura, and they therefore indicate that the common ancestral type of the Mollusca was, not a true Polyzoon, but simply a Polyzoon-like form. A lack of agreement in points of detail is therefore no more than we should anticipate. In answer to the second objection, that the Polyzoa multiply by budding, we may refer to the well-known law, that agamic vegetative multiplication is antagonistic to high evolution, and is accordingly replaced by true sexual reproduction in the higher forms of all classes of animals; as its presence, if it occurred in any of the true Mollusca, could not be regarded as proof of an affinity to the Polyzoa, its absence does not disprove such affinity. No one will attach much importance to the remaining objection,

that the Polyzoa are fixed; in fact, those which are developed from statoblasts are at first free, and swim by means of the cilia of the lophophore.

The similarity between the Polyzoa and true Mollusca, in general plan of structure, has long been recognized, but the attempts to connect the two groups through the Lamellibranchs are so evidently incorrect, that, led by the unquestionable affinity of the Polyzoa and Brachiopods to the Vermes, many geologists are now inclined to separate these lower forms from the Mollusca proper. As soon as we recognize that the Lamellibranchs are not to be regarded as typical Mollusca, and that all of the latter are to be traced back to a "Veliger," all difficulty seems to disappear, and it becomes plain, not only that the Mollusca and Molluscoida are related, but that they are connected so closely, that the advisability of such a division is very doubtful. We also obtain, at the same time, an explanation of the worm-like early stages of the embryo, exhibited by so many of the true Mollusca. The belief so firmly supported by nearly all zoologists a few years since, that the various branches of the animal kingdom are absolutely independent of each other, has been almost entirely overthrown by the accumulation of new facts, and the constantly increasing tendency to examine them in their bearing upon the theory of the evolution of life; and the union or junction of the Vermes and the Mollusca, in some manner, has already found a number of advocates.

Professor Morse, by his investigations upon the anatomy and embryology of the Brachiopods, has shown that, if we consider this group by itself, it must be placed with the Annelids. His investigations also show, with equal clearness, that the Brachiopoda are closely related to the Polyzoa, and we must therefore regard them as united by the "Veliger" to the true Mollusca. If we accept the view that the molluscan and vermian stems are thus united, the question, "Are the Brachiopods Worms or Mollusks?" will be regarded as nothing but a verbal discussion; for this class forms the connecting link between the two groups, and any sharp line of demarcation does not exist.

We are now prepared to form a provisional phylogeny of the Mollusca, which may be stated as follows:

The Brachiopods are derived from the Vermes; and from the Brachiopod stem, but from something very different from any known Brachiopod, the Polyzoa originated. From the Polyzoon stem, but not from any known Polyzoon, we have the Veliger. The true mollusks have originated as several offshoots from this Veliger stem. Of these the Scaphopods seem to be the least specialized, and in most respects nearest to the original proto-mollusk. The Pteropods are the representatives of another offshoot, to which the Cephalopods also seem to belong. The Gasteropods seem to represent several

distinct branches. The Prosobranchiata and perhaps the Heteropods being the descendants of one; the Opisthobranchs and Pulmonates of another; and the Chitons of a third. From one of these, or perhaps from the branch now represented by Dentalium, the Lamellibranchs seem to have been derived at a very early period, and to have diverged considerably from the ancestral form, becoming degraded in certain respects and at the same time specialized in others.

In this scheme all reference to the Tunicata is omitted, since it will be conceded by all embryologists that, whatever the affinities of this group may be, they are certainly not mollusks.

I have already referred to one serious objection to the view here advocated; that is, that it fails to account for the remarkable embryonic forms of certain Pteropods. Huxley has advocated the view that the Pteropoda and Dentalium have an annelidian ancestry distinct from that of the remaining Mollusca. This view would help us to understand the remarkable larval form of such genera as Pneumodermon, and at first sight would seem to present a way of escape from our difficulty. It fails to account for the perfect agreement between the veligers of the thecosomatous Pteropods and the Gasteropods, however, and thus introduces a difficulty at least as great as that which it removes. At present the safest plan seems to be that of waiting for more knowledge, bearing in mind the existence of this at present insoluble difficulty.—*From the Proceedings of the Boston Natural History Society*, Feb. 1876.

V.—*The Application of Photography to Micrometry, with special reference to the Micrometry of Blood in Criminal Cases.*

By J. J. Woodward, M.D. (U.S.A.).

Recent experiments in photographing the blood-corpuscles of man and other animals lead me to propose photography as affording the means of making comparative measurements more accurately, and with less expenditure of time, than can be done by any other method.

The plan I propose is simply as follows. The blood is placed on a glass stage micrometer and photographed with any convenient power, both blood and micrometer appearing sharply defined in the picture. The measurements are then made on the negative.

The stage-micrometer must be ruled on the upper surface of a piece of glass, and must have no thin cover. For mere comparative measurements any ordinary stage-micrometer in which the lines are equidistant can be made to answer by simply removing

its cover; but if absolute values are aimed at, the micrometer must be compared with a recognized standard, and its constant error ascertained. The stage-micrometer used in the present series of experiments is the same described in a former paper.* It is ruled in hundredths, thousandths, and five-thousandths of an inch; and, so far as I have been able to ascertain by repeated comparisons of its parts with each other, the several divisions of each kind are equal throughout, five of the five-thousandths being equal to any one of the thousandths, and ten of the latter to any one of the hundredths.

On comparison with a standard scale belonging to the United States Coast Survey, by the contact method of Welcker, this micrometer proved, as I stated in the paper just cited, to have a constant error of + 1·945 per cent.; that is, its lines are very nearly 2 per cent. too far apart; and this correction must of course be applied in the measurement of the photographs, as it was in the measurement of the corpuscles as seen in the microscope, which were published in the paper referred to.

The micrometer having been selected, it may be used for the measurement of blood dried in a thin film, of fresh blood in the moist state, or of dried stains soaked out by any selected method. In the first case the fresh blood should be spread on the micrometer by means of the edge of a glass slide, as proposed by Dr. Christopher Johnston, of Baltimore. A camel's-hair pencil is sometimes used for the same purpose; but it is a clumsy device, which no one who has been initiated into the proper method will ever employ. The blood thus spread may be photographed as seen uncovered, with a dry objective; but the best results are obtained with immersion objectives, to use which the specimen must of course be covered with a suitable piece of thin glass (·005″ to ·008″ thick).

After two or three negatives have been made from the sample first selected, it is washed off, and a fresh specimen substituted, and so on. If it is desired to photograph fresh blood, a drop is put on the micrometer, a thin cover (of the thickness named) dropped on and allowed to press out the blood to the thinnest possible film. Excellent results may thus be obtained with the micrometer, almost as sharply defined as in the case of dried blood. If dried stains are to be examined, as in criminal cases, the fragments of dried blood are to be soaked out, by any of the approved methods, on the same micrometer, just as if on an ordinary glass slide; and when a satisfactory preparation has been made, it may be photographed, together with the micrometer, as in the former

* J. J. Woodward, "On the Similarity between the Red Blood-corpuscles of Man and those of certain other Mammals, especially the Dog," &c., 'American Journal of the Medical Sciences,' January 1875, p. 151, and the 'Monthly Microscopical Journal,' February 1875, p. 65.

cases. For the purpose of soaking out such stains, I myself very much prefer to use a strong solution of caustic potash, as described by Virchow in 1857.* This reagent dissolves the fibrin and sets the red blood-corpuscles free without materially modifying them.

In order to obtain photographs of the specimens thus prepared, which shall at once be well defined and magnified sufficiently to render differential measurements easy, it is advisable to use immersion objectives of high power. I have employed the Powell and Lealand $\frac{1}{16}$th, and the $\frac{1}{18}$th of Tolles, belonging to the Army Medical Museum, for this purpose, and, rejecting all eye-pieces and amplifiers, have aimed to obtain a magnification not less than 1000 diameters by distance alone. Of the two objectives named, the one by Tolles gives somewhat the sharper images, and has the flatter field. I have therefore used it for most of the work. It is only just to repeat here that, although correctly named a $\frac{1}{18}$th, this objective is of lower power than the wet front of the Powell and Lealand $\frac{1}{16}$th, belonging to the Museum, which is really a $\frac{1}{19}$th, although the dry front of the same combination is a $\frac{1}{16}$th. The chief difficulty in making photographs of blood-corpuscles with this high power is to avoid diffraction fringes in the images; but this can readily be done by following the method which I have explained in detail in my paper on "Photographing Histological Preparations by Sunlight." † This method is extremely simple, and offers no difficulties to anyone who has acquired a reasonable degree of skill in microscopical manipulation. It is my desire to put it at the service of all sincere workers in this direction, and I cannot therefore view with indifference the publication of imperfect substitutes, which can only serve to waste the time of anyone who may be misled into employing them. For this reason I feel it a duty to warn the reader against attempting to follow the methods described in two recent papers on this subject, published in the 'Philadelphia Medical Times.' ‡ It would be waste of time to criticise these papers in detail. The methods described in them appear to have been devised without any intelligent consideration of the optical principles involved. I was not therefore surprised, on seeing some of their author's photographs of blood-corpuscles with high powers, referred to in his second paper, to find them lacking in definition, full of diffraction fringes, and, by reason of these faults, unfit for measurement or any other serious

* R. Virchow, "Ueber die forensische Untersuchung von trockenen Blutflecken," Virchow's 'Archiv,' bd. xii. s. 334.

† Surgeon-General's Office, July 13, 1871. Reprinted in the 'American Journal of Science and Arts,' October 1871, p. 258, the 'Monthly Microscopical Journal,' October 1871, p. 169, and the British Journal of Photography, October 27, 1871, p. 507.

‡ Carl Seiler, "Photographic Enlargements of Microscopical Objects," 'Philadelphia Medical Times,' June 5, 1875, p. 563. By the same: "High Powers in Micro-photography," *op. cit.*, February 10, 1875, p. 249.

use.* Nor was I surprised to read, near the end of the second paper, that "no one who has not tried it can form an idea of the difficulties attending the application of high powers to micro-photography, and a single tolerably good negative is often the only result of a hard day's labour."

By my own easy method, on the other hand, an average morning's work produces from fifteen to eighteen successful negatives. The time of exposure with the objectives named above, arranged to magnify the blood-corpuscles about 1000 diameters, is usually less than a second. A heliostat is therefore not *necessary* to obtain satisfactory results, as I have several times shown by taking such pictures quite as well without it as with it. The heliostat, however, is a great time-saver, since without it the light must be readjusted for every picture. I recommend anyone about to experiment in this direction to procure one. An instrument which will answer every purpose can be extemporized for a few dollars out of a bit of looking-glass and the works of a Yankee clock; but even the cost of the admirable heliostat of Silbermann, as made by Dubosq, is only five hundred francs, which will be more than repaid by the time saved during the first year, unless the time of the experimenter is not worth as much as that of an ordinary day-labourer.

I also find it economical to employ a professional photographer in my dark-room, and I recommend others to do the same. By this plan the microscopist is left free to devote his sole attention to procuring the best possible optical images, while the photographer has nothing to consider but the best possible chemical work. A much greater quantity of good work can be produced in this way, in a given time, than is possible if the microscopist undertakes to do the photography himself. In this case he almost certainly sacrifices the optical part of the work to the photographic, or *vice versâ,* and a reasonable degree of success is attained only by a wasteful expenditure of time.

Satisfactory photographs having been obtained, the corpuscles are next to be measured on the glass negatives. Paper prints are never exactly the size of the negatives. They spread a little if rolled; if not rolled, they may prove either a little larger or a little smaller than the negatives. Moreover, this spreading or contraction does not take place equally in all directions, and is sometimes quite irregular. It is most accurate therefore to measure on the negatives. For this purpose I lay the negative, with its varnished film uppermost, on the ground glass of an ordinary photographic re-touching frame, illuminated from behind by a mirror, and measure

* Since writing the above, I have seen a photograph of blood by Dr. Seiler which is free from diffraction. It is of course taken with a much *lower* power than the others. Even this picture is *not sharp,* as it would have been had a proper method been used.

the corpuscles under a magnifying glass by means of a transparent scale ruled in hundredths of an inch on a thin strip of horn. The accuracy of this scale is of course determined by comparison with a standard. To avoid parallax, I turn the ruled side of the scale down so as to bring it in contact with the varnished film. It will very generally be found that the dried corpuscles are not perfectly round; the longest and shortest diameters must then be measured, and the mean taken. As the image of each corpuscle is measured, a dot of water-colour is put on it, that it may not be measured a second time. This is readily washed off when the work is done. Extremely deformed corpuscles, and those which are obviously turned on edge, are not measured; but no others are omitted. As each corpuscle is measured it is entered on a check-list, which shows, when all on the negative have been measured, the number of corpuscles of each size. The sum of all the values, divided by the number of corpuscles measured, gives the average size of the images of the corpuscles in one one-hundredths of an inch. This average size, divided by the true magnifying power, gives the true average size of the corpuscles. To find the true magnifying power, I measure the distance of the lines of the micrometer from centre to centre on the negative, and divide by their true distance apart on the micrometer (that is, their nominal distance apart corrected by the ascertained constant error of the micrometer). The number of corpuscles on each negative, in my experiments, has ranged, with a single exception, from 50 to 175. Had the power always been just 1000 diameters, the measurements in one one-hundredths of an inch would have corresponded to one one-thousandths of an inch, and could be relied upon as correct to that figure. That is, writing the results in decimals of an inch, any error of observation would have been less than one significant figure in the fifth decimal place. With slightly higher magnifications, the results are still more accurate. When it comes to computing the mean of from 50 to 175 such measurements, it can hardly be questioned that it is proper to carry out the mean results one decimal further, and it is not extravagant therefore to claim that the computed results are correct to the sixth significant figure. In the appended table, therefore, I give the average size of the corpuscles represented on each negative in millionths of an inch (together with the equivalent value in millionths of a millimeter, obtained by computation).

I feel justified in claiming for the method above detailed, that it requires less time for an equal number of measurements, and that it is more accurate than any of the methods heretofore employed for the micrometry of blood-corpuscles. I also claim for it that it is capable of useful application in the micrometry of many other objects for which great accuracy is desirable.

As to the time required, I suppose that twenty-five to fifty negatives, containing from 50 to 175 corpuscles each, can be made

and the corpuscles measured in less than a quarter of the time necessary to measure the same number of corpuscles in the microscope by means of a glass eye-piece micrometer, and in less than a tenth of the time necessary if a cobweb micrometer be used.

As to accuracy, I have already mentioned the advantages of the plan proposed. I have next to refer to certain sources of inaccuracy in the method ordinarily employed. This method consists in the use of an eye-piece micrometer to which values are first given by focussing on a stage-micrometer and comparing the two sets of lines; the stage-micrometer is then removed and replaced by the slide to be measured. But the values thus obtained are only true so long as all the optical conditions under which they were procured are rigorously maintained; and unintentional errors may be introduced in various ways. Especially must it be noted that the least alteration of the cover-correction of the objective, whether by accident or for the purpose of improving definition, will be found to modify the magnifying power of the objective, and of course to alter the value of the eye-piece micrometer. If, bearing this fact in mind, the observer first finds the best position of the screw-collar for the slide of blood to be measured, and then inserts the stage-micrometer, to give values to the eye-piece micrometer, he will very often discover, when he replaces the blood-slide and begins to measure it, that the cover is of different thicknesses in different parts, and that he must either change the correction or be content to measure the corpuscles as seen somewhat out of focus.

Besides this source of error, to which too little attention has been paid, there is another, which appears to have been altogether neglected. Most of the published measurements of blood-corpuscles have been made on blood dried in thin films on glass. But however daintily this operation is performed, a large proportion of the corpuscles become more or less elliptical in drying. Yet in the micrometry of the blood-corpuscles, as practised hitherto, they are measured in only one direction. Under these circumstances inaccurate results are of course inevitable.

With the comparatively low powers used by most of those who have published measurements of the blood-corpuscles, this source of error was naturally overlooked: but with a thousand diameters and upwards it becomes evident enough. In making the measurements reported in my former paper, I endeavoured to escape this error by measuring only those corpuscles which appeared to be perfectly round. "Large and small forms were not searched for, but all the perfectly formed corpuscles brought into view by the movement of the stage were measured as they passed under the micrometer, without selection, until the required number was recorded." I am now satisfied that the larger corpuscles are more frequently deformed than the smaller ones, and that, by pursuing the plan I did, I measured an undue proportion of the smaller corpuscles, and thus

obtained averages somewhat less than the truth. In this way only can I account for the circumstance that the measurements now published are somewhat larger than those in my former paper, although the same micrometer was used, and with the utmost care.

Of course, since this source of error has been pointed out, it would be possible to measure the longest and shortest diameter of the elongated corpuscles in the microscope as well as on the photographs; but it would require a much greater expenditure of time.

It is my purpose, when time permits, to prepare a series of photographs of moist blood, and of blood soaked out from dried stains, as in criminal cases. Perhaps I ought not to go so far as to say that no expert who goes into court to testify about blood-stains deserves to be listened to by a jury unless he takes with him photographs of the blood examined on a stage-micrometer; but certainly it must be admitted that hereafter the most trustworthy expert-testimony in such cases will be that which is corroborated by photographic evidence. The general introduction of this severe method of recording accurately the facts observed, is the more desirable, because of late a spirit of exaggeration seems to have possessed certain experts, who either boldly claim, or in ambiguous language obscurely insinuate, that they possess the power of discriminating human blood from that of other animals in the dried stains which are submitted to examination in criminal cases.

The latest offender in this direction is Malinin,* who published last year a paper in which, by the measurement of corpuscles soaked out from dried stains, he claims to distinguish not only between human blood and the blood of many other mammals, but, under certain conditions, between human blood and the blood of any other mammal, even the dog. To make out his case, Malinin assumes the invariable accuracy of Carl Schmidt's † mean values of the diameter of the blood-corpuscles of man and certain mammals, which he republishes without credit, as if they were his own. It is precisely this assumption of invariability which leads him astray. The truth is that not only do the individual corpuscles in every drop of blood vary considerably in size, but, as might be anticipated from this very fact, the average size obtained by measuring a limited number of corpuscles (50 to 175, still more in the case of but 10 to 50, as usually practised) varies considerably, not only between different

* Malinin, "Ueber die Erkennung des menschlichen und thierischen Blutes in trockenen Flecken in gerichtlich-medicinischer Beziehung," Virchow's 'Archiv,' bd. lxv. (1875), s. 528.

† Carl Schmidt, "Die Diagnostik verdächtiger Flecke in Criminalfällen." Mittau und Leipzig, 1848. When I published my former paper, as I then stated in a footnote, I had not been able to see a copy of this paper; but one has since been received at the Library of the Surgeon-General's Office.

individuals, but also between different parts of the very same drop of blood.

Now it is true, as was shown by the measurements of human and dog's blood, published in my former paper, that the mean diameter of the corpuscles in a given sample of human blood is often rather larger than the mean of a sample of dog's blood selected for comparison. I may even go further, and say that the average of all the measurements of human blood I have made is rather larger than the average of all the measurements of dog's blood. But it is also true that it is not rare to find specimens of dog's blood in which the corpuscles range so large that their average size is larger than that of many samples of human blood. This was clearly shown by the measurements published in my former paper, as it is by those which are appended to the present paper, and will be by any fair series of comparative measurements.

Thus I note here with pleasure that, since the publication of my former paper, Professor Gulliver, whose measurements of blood-corpuscles are those most frequently cited in English works, has published an extended and revised table of measurements of the blood-corpuscles of vertebrates, prefaced by some remarks, in the course of which he expressly affirms the futility of attempting to distinguish human blood in criminal cases.*

I conclude this paper with a table of measurements of the red corpuscles of man, the dog, and the guinea-pig, as dried in thin films on the glass micrometer, and measured by the photographic method above described. A print of each negative accompanies this paper, † and copies of a selection of them will shortly be sent to convenient places in the larger cities, with the view of making them accessible to those who may be interested in this question.

The table gives the number of corpuscles measured on each negative, the diameter of the maximum and minimum, and the mean. The maxima and minima are given, like the mean, in millionths of an inch, because, although the measurements were

* George Gulliver, "Observations on the Sizes and Shapes of the Red Corpuscles of the Blood of Vertebrates," &c., 'Proc. of the Zoological Society of London,' June 15, 1875, p. 474. The passage referred to in the text is on page 484: "As before noticed, the magnitude of the corpuscles in a single species, not excepting the human, is liable to variations within certain limits; and there commonly appear in one field of vision of the same corpuscles differences amounting to at least one-third larger and smaller than the average. Hence, as regards the medico-legal question, however truly a careful observer (Dr. Joseph G. Richardson, 'Monthly Micros. Journ.' Sept. 1874) may have distinguished, by comparative measurements of the corpuscles, stains of human blood from those of the sheep or ox, this kind of diagnosis, as Dr. J. J. Woodward observes ('Monthly Micros. Journ.' Feb. 1875), would be ineffectual in some probable and more possible cases. It should be borne in mind, too, that in the apyrenæmata" (i. e. the mammalia) "the membranous bases of the blood-disks, when deprived of their colour by maceration in water, are about a third smaller than the unaltered corpuscles."

† A series of admirable photographs were sent with the paper, and can be seen by anyone who is interested in the matter.—Ed. 'M. M. J.'

actually made in hundredths of an inch, which with about 1000 diameters would correspond to about hundred-thousandths, yet the measures when corrected by the true magnifying power seldom remained a whole number, and the fraction is expressed by an additional decimal.

The measurements of human blood are from twenty-two negatives, taken from nine drops of blood obtained from eight individuals, the whole number of corpuscles measured being 1766. The maximum size measured was 396 millionths of an inch in diameter. But two corpuscles of this great size were measured. The smallest corpuscle measured was 216 millionths of an inch in diameter, and but a single corpuscle of this minute size was measured.

The number of corpuscles on each negative ranges from 50 to 140, except on a single negative, which presents only 26 corpuscles. This happens to be a group of large corpuscles, the mean diameter of which is 343 millionths of an inch, being the largest average diameter obtained on any negative. The smallest average was 309 millionths of an inch, being the mean of 90 corpuscles.

The measurements of dog's blood are from thirteen negatives, taken from five drops of blood, each from a single individual. The largest corpuscle measured was 378 millionths of an inch in diameter; the smallest, 237 millionths of an inch. The negatives contain from 80 to 175 corpuscles each, the total number of corpuscles measured being 1571. The largest average size for any negative was 340 millionths of an inch, being the mean of 100 corpuscles. The smallest was 296 millionths of an inch, being the mean of 111 corpuscles. It will be observed that on seven of the negatives of dog's blood the corpuscles have an average diameter smaller than the corpuscles on any of the negatives of human blood, while on the other six the average size of the corpuscles proves to be larger than the smallest average for human blood, and the largest average on any one negative of dog's blood exceeds that for any negative of human blood except the very largest. I call attention also to the very diverse averages obtained with both the human and dog's blood from different parts of the very same drop.

The measurements of guinea-pig's blood are from four negatives only, made from different parts of a single drop of blood. The total number of corpuscles measured was 401. One of the negatives gives an average one-millionth of an inch smaller than the smallest average for human blood; all the others give averages larger than the smallest for human blood. The variations in size are not so great as in the negatives either of dog's blood or of human, but, as they are taken from a single drop, it can hardly be assumed that this is a characteristic feature of the blood of the guinea-pig. I think no one could have told from the examination of this drop of blood whether it belonged to the guinea-pig, the dog, or man.

Table I.—Measurements of Human Red Corpuscles from Eight Individuals.

	No. of Corpuscles measured.	Diameters of Human Blood.			
		Decimals of an English Inch.			Decimals of a Millimeter.
		Maximum.	Minimum.	Mean.	Mean.
1. Drop H, Neg. 849 ..	90	·000363	·000255	·000309	·007848
2. Drop G, „ 846 ..	55	·000353	·000245	·000311	·007899
3. Drop E, „ 835 ..	70	·000339	·000261	·000312	·007925
4. Drop D, „ 828 ..	140	·000346	·000255	·000314	·007975
5. Drop H, „ 848 ..	50	·000343	·000274	·000315	·008001
6. Drop C, „ 824 ..	50	·000337	·000273	·000316	·008026
7. Drop G, „ 840 ..	81	·000372	·000265	·000316	·008026
8. Drop G, „ 841 ..	104	·000363	·000255	·000317	·008052
9. Drop H, „ 847 ..	80	·000363	·000255	·000319	·008102
10. Drop D, „ 827 ..	90	·000364	·000218	·000320	·008128
11. Drop A, „ 820 ..	75	·000359	·000230	·000326	·008280
12. Drop B, „ 822 ..	105	·000368	·000258	·000326	·008280
13. Drop I, „ 854 ..	80	·000353	·000294	·000326	·008280
14. Drop C, „ 823 ..	75	·000360	·000261	·000326	·008280
15. Drop C, „ 825 ..	75	·000382	·000282	·000327	·008306
16. Drop B, „ 821 ..	105	·000368	·000285	·000327	·008306
17. Drop A, „ 818 ..	80	·000359	·000278	·000328	·008331
18. Drop I, „ 855 ..	70	·000382	·000284	·000331	·008407
19. Drop D, „ 826 ..	95	·000371	·000285	·000334	·008483
20. Drop F, „ 837 ..	60	·000396	·000216	·000335	·008509
21. Drop A, „ 819 ..	110	·000396	·000276	·000337	·008560
22. Drop E, „ 836 ..	26	·000378	·000288	·000343	·008712

Table II.—Measurements of Red Corpuscles of the Dog from Five Individuals.

	No. of Corpuscles measured.	Diameters of Dog's Blood.			
		Decimals of an English Inch.			Decimals of a Millimeter.
		Maximum.	Minimum.	Mean.	Mean.
1. Drop A, Neg. 815 ..	111	·000352	·000257	·000296	·007518
2. Drop B, „ 830 ..	107	·000346	·000237	·000296	·007518
3. Drop E, „ 859 ..	120	·000363	·000245	·000298	·007569
4. Drop B, „ 829 ..	175	·000346	·000255	·000298	·007569
5. Drop E, „ 860 ..	120	·000353	·000255	·000301	·007645
6. Drop A, „ 816 ..	120	·000342	·000247	·000305	·007747
7. Drop C, „ 831 ..	152	·000355	·000246	·000308	·007823
8. Drop C, „ 833 ..	140	·000359	·000258	·000310	·007874
9. Drop D, „ 857 ..	100	·000353	·000265	·000310	·007874
10. Drop D, „ 858 ..	111	·000353	·000255	·000310	·007874
11. Drop A, „ 817 ..	80	·000361	·000238	·000315	·008001
12. Drop C, „ 832 ..	135	·000350	·000276	·000317	·008052
13. Drop C, „ 834 ..	100	·000378	·000270	·000340	·008636

TABLE III.—MEASUREMENTS OF RED CORPUSCLES OF THE GUINEA-PIG FROM ONE INDIVIDUAL.

	No. of Corpuscles measured.	Diameters of Guinea-pig's Blood. Decimals of an English Inch. Maximum.	Minimum.	Mean.	Decimals of a Millimeter. Mean.
1. Drop A, Neg. 852 ..	111	·000363	·000265	·000308	·007823
2. Drop A, „ 850 ..	100	·000372	·000265	·000310	·007874
3. Drop A, „ 851 ..	90	·000353	·000253	·000313	·007950
4. Drop A, „ 853 ..	100	·000382	·000265	·000314	·007975

I propose to make a number of other negatives of the blood of the dog and the guinea-pig at an early day, with a view to additional measurements; but those now published are, I must think, quite sufficient to demonstrate the reckless temerity of those who would attempt to discriminate human blood, even on nicely dried slides prepared with every possible care to preserve the shape of the corpuscles.

NEW BOOKS, WITH SHORT NOTICES.

The Anatomy of the Lymphatic System. By E. Klein, M.D., Assistant Professor at the Laboratory of the Brown Institution. II. The Lung. Smith and Elder, 1875.—The reason why Dr. Klein's book has been unnoticed for so long a period is simply this, that it has been placed in the hands of one gentleman whose illness rendered his reviewing the work absolutely out of the question; and then it passed to a second individual, who has unfortunately left town without performing his labours. And these circumstances, although they may appear somewhat strange to one unacquainted with the difficulty of finding the person who can review a histological volume, will not at all astonish those who are informed on the subject.

The work now before us is the second volume of Dr. Klein's treatise on the Lymphatic System, the first volume of which, it will be remembered, we noticed at some length. And, in the first place, let us say of it that it is almost without exception the finest essay on a special branch of microscopic anatomy that has been published in this country for many years. Its illustrations are most numerous, and are of course of such an excellence that we must confess that they are not the work of an English house, but that they have been executed at Leipsic. And, from our experience of English microscopical work, we cannot say that we have been ever satisfied with the figures accompanying it, except in the case of Dr. Beale's works, which were executed, as every histologist is aware, under peculiar circumstances. Dr. Klein's drawings are, however, most admirably represented.

The present work, which consists of upwards of 70 pages of large 8vo matter, deals with the microscopical structure of the healthy and diseased lung; but of course it has only to do with special portions of the histological arrangement. Thus it treats of the endothelium and also the matrix and the lymphatic system of the pulmonary pleura; then with the lymphatic system of the bronchi, and also with the perivascular lymphatics of the proper lung-tissue. In its pathological portion the author speaks of the pleura in acute and chronic inflammation, of changes in the lung proper in artificial tuberculosis of guinea-pigs, and finally of acute tuberculosis in man. And on all these subjects he has dealt fully and fairly with work that has been done before, and he adds a considerable amount to our knowledge from his own observation. And of the latter it must be said that the researches, especially the pathological, appear to have been most carefully carried out. Of course there are points in Dr. Klein's work which are still open to discussion, and we think that no one would be more willing to admit this than the author himself; and in this respect we may indicate two questions which may be raised: first, as to whether the subpleural lymphatics stand in direct *open* connection with the pleural cavity? and the second is, as to whether some of the so-called lymphatics may not be merely connective-tissue channels which have a certain communication with each other and no connection whatever with the undoubted lymphatic channels? However, these are merely queries. The general character of the work which

the author has done on the healthy lung may be gathered from the following passage:

"The radicles of the lymphatic system of the lung are distributed over three different parts: (1) the walls of the alveoli; (2) the walls of the bronchi; and (3) the pulmonary pleura. The first system is represented by irregular lacunæ and anastomosing canals, being the spaces for the bronchial connective-tissue corpuscles; it gives origin to lymphatic vessels which are provided with a special endothelial wall. . . . . The second system of radicles, viz. that situated on the wall of the bronchi, is represented by irregular lacunæ and anastomosing canals in the mucosa, on the more external parts. . . . . both contain, or more correctly speaking are lined, by the connective-tissue corpuscles. . . . . The third system of radicles, viz. those on the pulmonary pleura, are also interfascicular lacunæ, communicating with each other by a few canals; each lacuna is lined by a connective-tissue cell-plate."

Of course but the merest outline can be gathered from the above quotation, therefore we trust our readers will obtain the work itself. With regard to the pathological portion, Dr. Klein is here standing almost on virgin soil, at least as far as English workers are concerned, with the exception of the writings of Dr. W. Fox and Dr. Sanderson. The German authorities are, however, copiously referred to, and their different views are noted. It is to be observed, however, that Dr. Klein does not agree with all their opinions, but in some cases expresses his own ideas very distinctly. This is more especially evident in regard to Hering's views on the subject of miliary tuberculosis in man. The latter is of opinion that the so-called giant-cell "corresponds merely to a granular substance filling up the lumen of a lymphatic vessel; and what is generally described as the nuclei of the giant-cell, to be identical with the endothelial cells (!) of that vessel." To this Dr. Klein replies that "I have occasionally seen a giant-cell of a distinct tubular shape. But this I think may simply mean that the giant-cell has, from some reason or other, grown into that shape. Besides, the disposition of the giant-cells in many a tubercle of the lung is in conflict with their being identified with lymphatics."

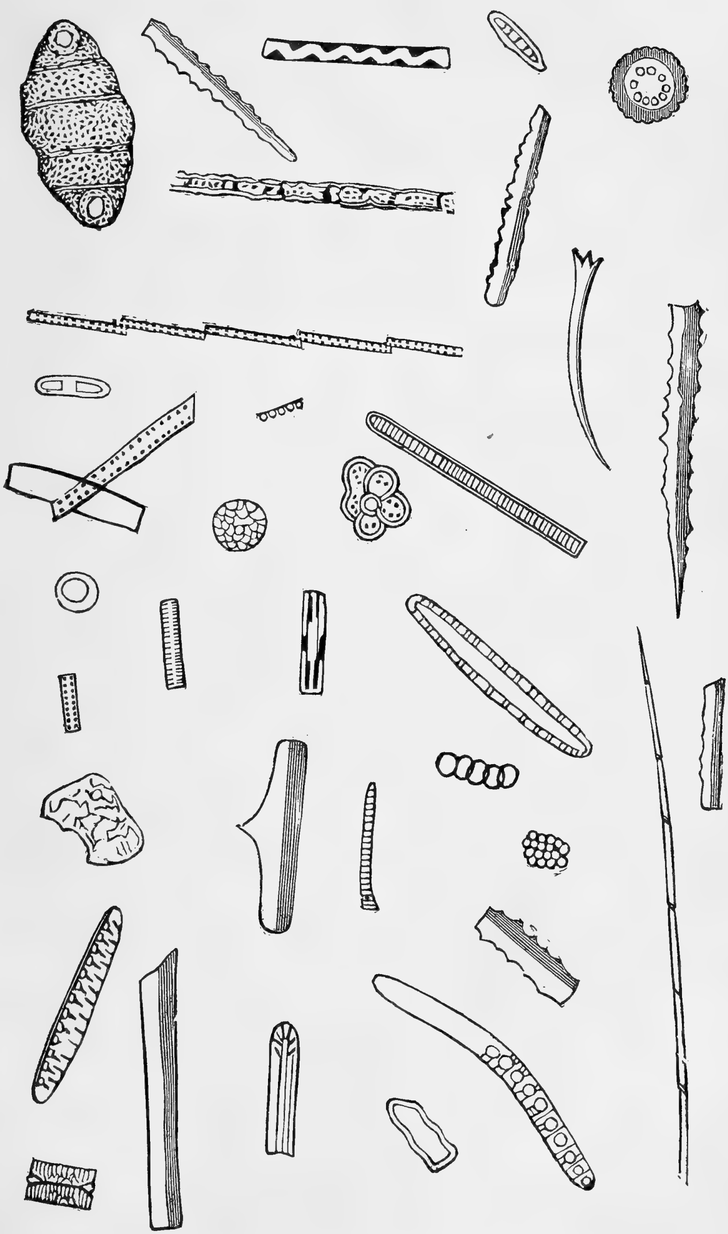
Throughout, the work is excellent in its nature. We note too that the author's style is much less German than it was heretofore. Indeed, with a few exceptions, the book might have been written by an Englishman. And while we are well satisfied with Dr. Klein's labours, we must not forget to say a word in praise of his publishers, who have issued the volume in a most thoroughly creditable style.

PROGRESS OF MICROSCOPICAL SCIENCE.

Diatoms in Infusorial Earth being absorbed by Roots of Corn.—In a paper published in the 'Quarterly Journal of Science' for July, and written by Dr. W. B. Wahl, on the subject of infusorial earths, the author refers to this question, and gives the accompanying Plate CLV. (which has been kindly lent us by Mr. Crookes, the editor). Dr. Wahl says that the manufacturers, to demonstrate the availability of the

Forms of Diatoms found in Col. Kunkel's Straw.

MAGNIFIED 300 DIAMETERS.



silica in the form in which they employ it, have actually succeeded in proving beyond question the highly interesting and novel fact that the very minute skeletons or shells of which the infusorial earth is mainly composed are carried up *as such* into the body of the plant itself. Upon this point the following gleanings from an investigation conducted by Professor P. B. Wilson will be read with interest.

This chemist subjected to a microscopical examination the straw from the wheat-fields of Colonel J. B. Kunkel, of Frederick County, Maryland, which had been fertilized by the silicated phosphate, his purpose being to make "a more complete investigation into the silicious structure of the stalk, in determining whether the infusoria passed directly as such into the sap-cells, to be carried forward by capillary force, and to finally assume their functions—the formation of the epidermal shield for giving strength to the straw, to withstand the destructive force of high winds and beating rains, as well as a protection against the attacks of parasites.

"In making these investigations thorough precautions were observed to cleanse the straw from all accidental impurities by washing and gentle friction, not sufficient, however, to destroy the epidermis. The organic matter was then removed by the prescribed methods, aided by my own experience.

"My labours," he continues, "have been amply rewarded by one of the most enchanting views that has ever fallen to my lot to behold through twenty years of varied scientific investigations. When the epidermal silicious coating was adjusted upon the field of the microscope, some thirty-six forms of the Diatomaceæ, which I have carefully sketched, were observed (see engraving, magnified 300 diameters) where perfect disintegration has been produced. When the structure to a great extent is retained a marvellous interlacing of these forms presents itself, sometimes side by side, at other times overlapping."

From this very interesting observation Professor Wilson advances a number of inferences, which are of sufficient interest to warrant their reproduction. He affirms that his investigation "overthrows all theories that have ever been advanced, that silica enters into plant structure in combination with the alkalies, the alkaline earths, or the earths proper. Chemical investigation led me to this conclusion some months since, now confirmed by that of the microscope.

"My mind was particularly impressed with the absence of the disk-like form, the *Actinocyclus Ehrenbergii* and the *Actinoptychus undulatus* in their perfect state in the straw, while the other forms are common both to the infusorial earth and the wheat. My conclusions are that the varieties mentioned are too large to enter the root capillaries, for on the field of the microscope they have three to four times the magnitude of the others. This I will fully investigate during the coming summer, by making accurate measurements of rootlets and diatoms, when I will be able to obtain stalks of wheat as grown in the fields, preferring this mode of investigation to *pot culture*, to disarm controversy, and to divest the investigation of all semblance of laboratory experiment.

"I have examined various specimens of wheat straw taken at

random from the market, but have failed to find a single diatom. This to a certain extent surprised me, when taking into consideration that they are found to a limited extent in Peruvian guano. The inference to be drawn is, that the soil was not fertilized by any material into which it entered as a constituent. I mention this to guide others who may make subsequent investigations from falling into error, in case occasional Diatomaceæ are observed, as being derived from other sources than the infusorial deposits.

"These microscopic investigations show the absence of other forms of silica, that is, in granular particles in the (Kunkel) straw, they being entirely replaced by diatoms. This leads to the conclusion that the diatom is the more acceptable for assimilation, and when sufficient infusorial remains are present, replaces any other divided form of silica. I have previously attempted to substitute silica for diatoms, as obtained from the decomposition of slags from iron furnaces, but have failed to derive any satisfactory results. This is due to its combination as a silicate; and when liberated by stronger acids, it agglutinates into masses too hard and large to be absorbed by the plant."

[We confess that the Plate hardly satisfies our mind.—Ed. 'M. M. J.']

[Many pages of our "Progress" have had to stand over till next number, in order to make room for Dr. Woodward's important paper.—Ed. 'M. M. J.]

NOTES AND MEMORANDA.

Salicylic Acid in Microscopy.—On this subject the following note appears from the pen of Mr. A. Mead Edwards, in the 'American Journal of Microscopy' (June 1876). He says:—"I have been waiting anxiously from day to day to see if some one would not announce the fact that in salicylic acid, the marvellous disinfectant and preservative, the microscopist had a firm and valuable friend. But as no one has said anything as to its value as a preservative of microscopic objects, I have felt it my duty to come forward and say my say. Towards the end of 1874, as soon as I saw the announcement of the wonderful properties of this substance, I tried it in medicine and in microscopy, and in both fields found it of inestimable value. I put up some casts of uriniferous tubules obtained from a severe case of nephritis, and to-day those specimens are in as good a condition as they were the day I put them in clear water, with a few grains of salicylic acid added. Every medical microscopist, at least, knows how perishable casts are, and will, I feel sure, thank me for this hint. Leucorrhœal discharges put up in 1874 do not appear to have changed in the least; and if salicylic acid will preserve such things as these, what will it not do? At present I am experimenting with it on various substances, and although it seems to alter the colour of most vegetable tissues, yet the softer and more perishable parts are so nicely preserved, that I feel that in it the working microscopist has a medium of greater value

than any other preservative he possesses. The beautiful little *Volvox globator* can be kept in an almost unaltered condition in a cold, saturated solution of salicylic acid; and I have made a truly elegant preparation of Volvox by first immersing it for a few minutes in Beale's carmine staining fluid, and then mounting it in the salicylic acid liquid. Details are brought out in this way that are difficult to see in the living animal. Desmids, likewise, seem to keep well in the same preservative, and diatoms, with which I am at present experimenting, seem not to change materially after immersion in salicylic acid solution for a week."

Histological Micro-photographs.—Our readers will be glad to learn that a work is promised by an American house, entitled 'Micro-photographs in Histology, Normal and Pathological,' by Carl Seiler, in conjunction with J. Gibbons Hunt, M.D., and J. G. Richardson, M.D.; to be published in twelve numbers.

A New Adjustment for Cox's Turn-table.—An American contemporary states that a slide may be, by this turn-table, centered for width only, by laying it on the table at right angles to the line of the spindle and placing triangles of brass, or even cardboard, between it and the clutches which are designed to hold the corners of the slide. When thus arranged, the slide may be slipped so as to bring different parts of its median line successively to the centre of the apparatus, and thus a series of cells may be made upon the same slide, or any desired group of cells may be made by using a variety of unequal triangles. For common use, the two triangles should be exactly alike, should be right-angled, and should have the sides adjoining the right angle one inch in length. Such pieces may be cut from sheet brass about the thickness of an ordinary glass object-slide. These triangles may also be used, with the addition of a few cardboard blocks, for the purpose of decentering, in refinishing old slides that have not been accurately centered.

PROCEEDINGS OF SOCIETIES.

Quekett Microscopical Club.

Ordinary Meeting, June 23, 1876.—Dr. Matthews, F.R.M.S., President, in the chair.

The usual nominations took place for President, officers, and four members of committee. Henry Lee, Esq., F.L.S., &c., was nominated President for the ensuing year.

A communication from the Rev. J. Bramhall was read, describing his new oblique illuminator, and explaining its principle and mode of use.

A paper by Mr. James Fullagar, on *Tubicolaria najas*, was read, giving an interesting description of a large number of observations of this rotifer, from the egg to maturity. These observations were made upon carefully isolated specimens. A curious fact in the life history of this rotifer was that it did not hatch out of the ovum, but

developed from it, so that the outer case of the ovum became the outer skin of the animal. The paper was illustrated by numerous beautifully executed drawings.

Mr. Fitch exhibited and described a rare species of *Arenurus* (*A. caudatus*), and presented a mounted specimen to the club.

Annual Meeting, July 28.—Dr. Matthews, F.R.M.S., President, in the chair.

The Secretary read the eleventh annual report of the committee, showing that there was no diminution in the prosperity of the club. The work during the past year had been above the average; several interesting papers had been contributed by members and friends, the excursions and the "gossip nights" had also been well attended. The number of members was 540.

The President then delivered the annual address. Commencing with the discussion of the advisability of limiting the area of our pursuits, and the wisdom of devoting ourselves to one branch of science, and of concentrating our energies upon a single subject, he proceeded to consider the effect of such a speciality upon the mental faculties, and the increased value of the contributions so made to science. He then passed on to the consideration of the future of microscopy with respect to its claim to be treated either as a science or an art; and after giving sundry definitions as to the relations of art and science, he drew the conclusion that microscopy stood on neutral ground between art and science, that it had claims upon various branches of science by reason of the great assistance rendered by it, and that its students were entitled to be considered as experts, and were worthy of recognition as such, for it was mainly by their aid that microscopical appliances and manipulation were kept on a level with the ever-increasing requirements of scientific research.

The President then presented the testimonials awarded under the donation made by Mr. Frank Crisp. These consisted of objectives, books, and apparatus, and had been awarded to Mr. W. Cole for his paper on *Sphærularia bombi*, to Mr. A. Hammond for his paper on the Metamorphoses of the Crane-fly and the Blow-fly, and to Mr. R. P. Williams for his improved freezing microtome.

Votes of thanks were then passed to the President and officers, and to the Council of University College, who had so long granted permission to hold the meetings in their library.

The ballot then took place, and Henry Lee, Esq., F.L.S., &c., was duly elected President of the club for the ensuing year. Upon his election Mr. Lee took the chair, and after a short address conducted the business of the ordinary meeting, which was merely of a formal character.

Victoria Microscopical Society.

The Microscopical Society of Victoria held its monthly meeting on February 24. Mr. Ralph, the president, described and exhibited an interesting and supposed new conferva found in a stagnant pool at Bulleen. The plant was notable for being composed of radiating fibres, with branches always dividing into two, and connected together in globular clusters, the interstices of which were

inhabited by rotifers. Mr. F. Barnard read some observations on a fungus infesting the leaves of one of the eucalypti, and not known to have been described. He also showed the specimen by means of his new imported microscope by Ross. Mr. Sydney Gibbons then addressed the meeting on "Methods of Modifying Light" for the relief of the eyes in microscopic observations, and showed some simple and effective contrivances for the purpose. He recommended the use of very pale cold blue glass, not purple, applied to the diaphragm and as a removable cap over the eye-piece. After experimenting on the subject, he preferred these appliances to all others.

The Microscopical Society of Victoria held its monthly meeting on March 30, the president (Mr. T. S. Ralph) in the chair. Mr. T. Burrows, of Hawthorn, was announced as the new hon. secretary. Mr. Sydney Gibbons read a paper on the use of the microscope in post-mortem analysis, chiefly addressing himself to the identification of structures found in the viscera of subjects under examination. In illustration, he described some curious cases, in which, by examining the various particles of food in the stomachs, and recognizing tissues, cells, and hairs of known plants, he had been able to throw considerable light on the cause of death. In one case, for example, of some cattle that died in a singular way, he found the stomachs highly inflamed and inordinately distended with matter in which, by the aid of the microscope, he identified among other things the structures of maize, and declared his opinion that death had been caused by the congestion often produced by gorging with succulent fermentable food, such as young maize, and accelerated by large draughts of water. The President then gave an interesting demonstration on the art of making cells for preparations and for microscopic observation. He illustrated his remarks by specimens and operations showing methods of working in glass to construct the apparatus required.

San Francisco Microscopical Society.

A regular meeting of the San Francisco Microscopical Society was held on Thursday evening [curiously enough, no date is given], President William Ashburner in the chair.

Mr. H. G. Hanks presented a very beautiful slide, being a section of chlorite in quartz, which, viewed with the spot lens and an inch and a half objective, was a most interesting object. The convolutions and peculiar crystalline form of the greenish chlorite imbedded in the transparent quartz were particularly noted.

A paper sent to the Society through the hands of Mr. J. P. Moore was read by that gentleman, who supplemented the same by exhibiting the spores of the fungus and also a twig showing the manner in which the tree on which it is found is injured by the parasitic growth.

Dr. Harkness' Paper.

Sacramento, *June* 29, 1876.

Dear Sir,—I have to-day forwarded for the Society's cabinet a specimen of Peridermium, a fungus which is attacking the small pine trees in the vicinity of Colfax, at which point Mr. Moore and myself

discovered it on the 26th of May last. On the 20th inst. I again visited the locality for the purpose of noting the changes which had occurred during the interval. The fungus belongs to the genus Peridermium, order Æcidacie, and appears both on the limbs and trunks of young trees of the variety *Pinus ponderosa*, generally forming a complete circle around the tree, its sporidia appearing as a zone of bright orange yellow.

The spores first germinate beneath the cuticle, which it destroys. Owing to the irritation of this presence, an abnormal thickness of the cambium is produced, which, in turn, gives place to an excessive growth of woody fibre.

This process being repeated from time to time, a large bulbous expansion is soon formed, so that as often occurs a stem of but an inch in diameter is enlarged to that of four or five.

Above this bulb the further development of the stem is retarded or arrested altogether, its place being supplied by a dense tuft of minute branches.

As no reference to this fungus is found in any of the books at my command, I am inclined to the belief that it is a new variety.

Yesterday I received a letter from Professor W. G. Furlow, of Cambridge, Mass., in which he says: "I send you a specimen of Peridermium, which is attacking the *Pinus ponderosa* of this vicinity. Do you find it in California?"

As the specimen sent by the Professor agrees in every essential particular with the one sent to the Society, it would appear that the fungus is attacking the forests in the eastern as well as the western portion of the United States.

But one other variety of Peridermium is yet known upon this coast, and that is found upon the foliage of the *Pinus insignis*, growing in the Golden Gate Park, San Francisco, in the month of January last. This variety we then identified as the *P. pini*, which so far has not produced any special damage.

In the vicinity of Colfax, the fungus appears to be limited to an area of but a few acres in extent. Within that area, however, it is destroying the young growth, and should it become generally disseminated, it bids fair to do great harm to the timber trees of this coast.

H. W. Harkness.

C. Mason Kinne, Esq.,
Secretary San Francisco Microscopical Society.

A motion by Mr. J. P. Moore, that the fungus be named by the Society *Peridermium Harknessii*, was carried unanimously, after which the meeting adjourned.

THE

MONTHLY MICROSCOPICAL JOURNAL.

OCTOBER 1, 1876.

I.—*Bastian and Pasteur on Spontaneous Generation.*

By Henry J. Slack, F.G.S.

(*Taken as read before the* Royal Microscopical Society, 1876.)

In the July 10 number of 'Comptes Rendus' is a paper by Dr. Bastian, "On the Influence of Physico-chemical Forces in the Phenomenon of Fermentation," intended to demonstrate, in opposition to the theory of atmospheric germs, that certain organic liquids contain complex chemical bodies which are capable of organization, and form different kinds of bacteria.

In support of these notions he recites experiments on urine caused to boil, and screened from the influence of atmospheric germs. To determine the production of bacteria in this urine he introduced potash and oxygen, and subjected it to 122° F. He states that, in numerous trials, urine previously rendered sterile, and heated as just stated, gave rise to bacteria. He found that a temperature of 122° F., though not generally considered favourable to fermentation, was so to the development of bacteria in urine and some other organic liquids.

In the autumn of 1875 he says he found that urine, normal, and acid, rendered sterile by ebullition, became fertile in two or three days when exactly saturated by potash, without other contamination, and after being exposed to an elevated temperature. He further states that he took the most minute precautions to avoid the influence of germs that might have been in the potash or on the walls of the vessels employed, as well as those which the air might carry.

He also states with regard to the influence of oxygen, that urine rendered sterile, neutralized by potash, and subjected to electric action through platina wires, gave remarkable results, fermenting rapidly at 122°, and becoming filled with bacteria in from seven to twelve hours. He considers these experiments overthrow the atmospheric germ theory, and cites Tyndall to the effect that bacteria germs are destroyed by a temperature of 212° maintained for a minute or two, as was the case with the fluids he used.

In 'Comptes Rendus' for July 17, M. Pasteur makes a very polite reply, tinged with a little irony, in the remark that the

heterogenists are more fortunate than the inventors of perpetual motion, in the lengthy attention they have received from scientific bodies. In the domain of mathematical sciences it is, he says, possible to demonstrate that certain propositions cannot be true, but natural sciences are less able to predict results. The mathematician may disdain to cast his eye upon an essay which has for its object squaring the circle, or perpetual motion; but the question of spontaneous generation excites public opinion, because it is impossible in the actual state of science to prove *a priori* that no manifestation of life can take place by a jump without the previous existence of a similar life.

When any observer announces that he has discovered the conditions capable of causing the spontaneous origin of life, he is sure of the prompt adhesion of the systematic supporters of his doctrine, and of raising a doubt in the minds of others who have only acquired a superficial knowledge of the subject. This is the more the case when an author, like Dr. Bastian, occupies an important position, has literary and dialectic talent, and brings forward conscientious researches.

During twenty years he has worked at this question, M. Pasteur says he has not been able to discover any life not preceded by a similar life. The consequences of such a discovery would be incalculable. Natural sciences in general, medicine and philosophy in particular, would receive an impulse of which no one could foresee the consequences, and if anyone succeeds in reaching such a result, he would welcome the happy investigator on his operations being proved. At present his attitude is one of defiance, as he has so often shown how readily able men make mistakes in this difficult art of experimentation, and what danger is connected with the interpretation of facts.

Let us see, he exclaims, whether Dr. Bastian has known how to escape these two rocks. He then cites the title of Dr. Bastian's paper and his chief remarks, and adds that he hastens to declare that the experiments described would usually give the results that are stated, and that he need not have operated at a temperature of 50° C., as at 25° or 30°, and even lower, boiled urine rendered alkaline by potash in a pure atmosphere becomes filled with bacteria and other organisms. If Tyndall, as Dr. Bastian says, thought this was not so, it must have been through forgetfulness. Dr. Bastian cannot be unaware that the experiments he has just communicated to the Academy, or at least experiments of the same kind, were made by me, and published in a memoir of 1862, entitled 'On Organic Corpuscles which exist in the Atmosphere: an Examination of the Doctrine of Spontaneous Generation.' I demonstrated in this paper (pp. 58 and 66) that acid liquids which always become sterile by a few minutes' exposure to 100° C., are

made fecund if we communicate to them a slight alkalinity. The novelty introduced by Dr. Bastian in having recourse to a temperature of 50° C. is only apparent, since this condition is superfluous. There is then between us only a difference in the interpretation of facts common to both. Dr. Bastian says these facts prove spontaneous generation, and I reply not at all, they only demonstrate that certain germs of inferior organisms resist a temperature of 100° C. in neutral and slightly alkaline solutions, doubtless because under such conditions their envelopes are not penetrated by the water, and that they are so if the medium in which they are heated is slightly acid. In reference to this I will recall that the workmen of Rouen, as M. Pouchet informed us, noticed that certain seeds attached to wool coming from Brazil germinated after four hours' exposure to boiling water, and M. Pouchet proved that when the germination occurred after such treatment the grains had preserved their natural size, their hard horny envelope not having been penetrated by water or steam; when the contrary was the case, germination was impossible. With regard to germs disseminated in atmospheric dust, I proved that they perish in an acid medium at 100° C., but they remain fertile if the medium is alkaline. (See p. 65 of my paper.)

If Dr. Bastian wishes to assure himself of his errors of interpretation he can easily do it. He obtains bacteria by saturating boiled urine with potash. I simply suggest that instead of employing an aqueous solution of potash, he should drop into the urine solid potash after making it red hot, or even only to 110° C. His experiment will then never succeed; that is, he will obtain no formation of bacteria in urine exposed to 30°, 40°, or 50° C. The conclusion he has drawn from our common experiments is thus inadmissible, for it would be absurd to pretend that the *primum movens* of life is in melted caustic potash. Such is the way of obtaining a decisive result. In one word, I only ask Dr. Bastian to eliminate the bacteria germs which were contained in the aqueous solution of potash he employs. If Dr. Bastian finds it difficult, from the apparatus he uses, and does not describe, to bring the potash to a red heat previous to cooling it, and dropping it as a solid into the urine, let him, instead of heating it to 100° C., heat it to 110° C., and he will then find sterility if he operates with vigorous accuracy. If he still preserves his doubts, let him suppress the preliminary condition of causing the urine to boil; for it is a remarkable fact that urine in its absolutely normal state as it leaves the bladder of a healthy man remains sterile if a certain quantity of potash is dropped into it, with the precautions I have described, in chapter iii. of my recent work on beer, to avoid contact with atmospheric germs. Dr. Bastian conscientiously

seeks the truth, and no alternative conclusion is possible. I entertain the firm hope that he will abandon his belief in spontaneous generation and in the proofs he supposes he has adduced.

M. Pasteur, at the close of his paper, stated verbally that although the urine of a healthy man contains no extraneous germs of organic bodies, that in most cases it comes into contact with such germs at the moment of its emission at the extremity of the urethral canal, or in the surrounding air. He also described the very simple apparatus he employed to repeat Dr. Bastian's experiments with decisive results. It is a pity no details of this are given in 'Comptes Rendus.'

Dr. Bastian's reply to Pasteur's criticism, and the latter's rejoinder, will be found in 'Comptes Rendus' for July 31 and August 7; they add nothing to the preceding.

II.—*The Markings of Frustulia Saxonica.*

By Samuel Wells, Mass., U.S.A.

(*Taken as read before the* Royal Microscopical Society, 1876.)

It may not be important whether this minute diatom is marked with longitudinal lines or not, but if the appearances of such lines, which have led to controversy, are to be taken as suitable tests for spurious lines, the question becomes one of considerable importance. I was formerly of the opinion that these lines resulted from diffraction, and in coming to that conclusion I was aided by Dr. Woodward's article in the 'Lens' of October, 1872.* Subsequently, with better objectives and improved methods of illumination, I changed my opinion, and satisfied myself that the longitudinal lines are as real as the transverse. In Dr. Woodward's communication to the Royal Microscopical Society,† he states that he still regards the lines as spurious, and enforces his opinion with some of his admirable photographs. These are arguments very difficult to answer, and outweigh the simple statement of many observers to the contrary. I have therefore not thought it useful to attempt to discuss the subject until I could support my opinions with arguments of the same kind—whether of the same force others may decide.

In order to guard against the possible charge that I have mistaken the coarser forms of the same species (Rhomboides) for the variety in question, I have selected for the photograph a particular frustule already on record, being the identical specimen numbered 18 on a Möller's Probe-Platte measured by Professor E. W. Morley, whose communication is published in the 'M. M. J.' of May last, p. 223. This Probe-Platte is the one marked C by him in his table of measurements on p. 226, and on this particular frustule he finds the transverse striæ to range from 81·5 to 82·7 in ·001″. I think, therefore, it may fairly be called a representative frustule of the *F. Saxonica*, although apparently somewhat larger than those selected by Dr. Woodward for his photographs.

The accompanying photograph ‡ of this frustule, marked A, shows the longitudinal lines over the whole surface. I find them to be 88 in ·001″, but do not claim great accuracy in the measurement. It will be noticed that this frustule is in balsam, while the lines are therefore fainter than they would be in a dry mount, yet the balsam eliminates the principal cause of the disturbing spurious lines. The midrib and margin of this species are quite thick, and

* Reprinted 'M. M. J.' vol. xiv. p. 279. † 'M. M. J.' vol. xiv. p. 274.

‡ The photographs sent, those of *F. Saxonica* and *A. pellucida*, rather bear out the author's views. They are, however, hardly of sufficient interest to demand a Plate; we have therefore placed them with the other photographs which Mr. Wells has sent to us, in the hands of Hon. Secretaries of the Royal Microscopical Society.—Ed. 'M. M. J.'

in very oblique light produce diffraction phenomena, which in some cases obscure the whole frustule, and usually leave but little of the real aspect perceptible; balsam, however, has a refractive power more nearly that of the silica, and in that medium the diffraction lines are quite within control.

The illumination which I have used in taking this photograph was obtained from Wenham's reflex illuminator, used in the manner first described by me in the 'Boston Journal of Chemistry,' and reprinted 'M. M. J.' vol. xiv. p. 30. Of course only very wide-angled objectives can be used with the reflex in this manner. The one I have chosen for the purpose is a Tolles' $\frac{1}{10}$th duplex front, made for me last year.

I send also, marked B, a photograph of the transverse lines of the same frustule, and one, marked C, of the *Amphipleura pellucida*, No. 20, on the same Probe-Platte, both taken with the same objective and illumination. They appear to indicate the beaded character of the striation.

I have seen no record of any photograph of the *Amphipleura pellucida* in balsam, except that of Count Castracane,* which he did not deem good enough to print from. Following the example of that careful observer, I use a prism, and take my photographs with the blue rays.

I send also other photographs taken with central light, which I shall be pleased to have presented to the Royal Microscopical Society, if you deem them of sufficient interest.

* 'M. M. J.' vol. v., p. 176.

III.—*Observations upon Mr. William A. Rogers' Paper "On a Possible Explanation of the Method employed by Nobert in Ruling his Test-Plates."* * By W. Webb.

(*Taken as read before the* Royal Microscopical Society, 1876.)

Mr. Rogers says: "The problem is naturally divided into two parts: (*a*) The mechanical operation of moving the plate to be ruled over given and equal spaces; (*b*) The operation of producing on glass, lines of varying degrees of fineness." Mr. Rogers then gives an elaborate description of his meritorious efforts to supply the exigencies of the first division "(*a*)", and modestly gives the results as "the outgrowth of his own experience."

If Mr. Rogers had availed himself of qualified assistance, the fact would have been ascertained that his instrument did not supply the primary necessity of elimination of friction, and that his "plate against which the precision screw works as a shoulder" is not merely unnecessary, and therefore a surplusage, but is positively detrimental in creating friction in the initial step of the work. Mr. Rogers might also have ascertained that this part of his instrument ought to consist only of three pieces—viz., the carrier for the glass, the screw to project the carrier, and the bed upon which the carrier moves, the bed having one end turned up at right angles and tapped for the screw.

Mr. Rogers may forget, or perhaps never have known, that in 1861 (before the Royal Microscopical Society of England had obtained its charter of incorporation) bands of lines after the manner of Nobert, *ruled without a screw*, were exhibited to the members.

He is a bold man who ventures to say by what means Nobert produces his wonderful and deservedly admired effects; not merely whether he produces them with or without the use of a screw, but whether with or without a diamond; as to which Mr. Rogers says, "The evidence seems quite clear that they are ruled with a diamond having a knife-edge."

How Nobert produces his lines is not so much the question as the laudable ambition to produce similar lines by some means without knowing Nobert's process, and Mr. Rogers is most certainly wrong in his assumption of a knife-edge, the cut from which, if not immediately covered with balsam and fixed on a slip, will sooner or later by the mere vibration of the earth end in disruption, and even if balsamed down, the time may come when the balsam itself starts from the glass. Nobert's are as good after a quarter of a century as when first cut, which is an impossibility with knife edge ruling unfilled in; and to fill in with balsam, without first blacking in, would be, in consequence of the minute

* Page 74, *ante*.

difference in the refraction of glass and of balsam, to utterly obliterate the work.

Mr. Rogers may be surprised to be informed that if he work with a diamond at all, it must be one which acts precisely as a cabinet-maker's plane acts when smoothing a board, and producing coiled glass shavings. The late Mr. Farrants tried, and the writer of these lines has tried, in vain to mount the glass spirals.

Micrometers are sometimes ruled with a knife-edged diamond as well as with other cutting diamonds, and with bort, but they are then filled in with black, covered with balsam, and fixed down.

When Mr. Rogers talks about what he designates as the grain of the glass (which he suggests may be the effect of the polishing), and which he says prevents his ruling both ways, Mr. Rogers may reflect upon the fact that English stage-micrometers are all ruled both ways.

A great patron of microscopology, Frank Crisp, Esq., of London, has a diamond engraving of the Lord's Prayer, in which the letters are smaller than the two hundred and ten millionth part of a square inch, at which size over fifty-nine [59] Bibles would be required to cover an inch; and Mr. Crisp has also several others, the largest of which would fill an inch with a thousand letters. Some of these are blacked in and balsamed down, while others are not blacked in or balsamed down, and are all produced by ruling straight and curved lines, *intersecting each other in every direction*, and not one has a broken or jagged edged line.

Mr. E. Wheeler, of Tollington Road, London, last year forwarded to the present writer a set of bands, after Nobert, to repair and remount, so as to obviate the effect of an accident, which lines, fourteen years after being *ruled without a screw*, and mounted dry, were still perfect in their gouge-like cut.

With regard to what Mr. Rogers calls the "periodicity of errors," and which that gentleman in emphatic italics attributes not to the screw itself but to the mounting of the screw, possibly he will confirm the experience of others if he take the head off the screw and make arrangements to reunite them in different positions in relation to each other, so that zero on the head would be coincident with different parts of the periphery of the screw at different times, and rule a set of lines with the screw and head in each of the different relative positions, he would possibly find the periodicity of errors would be exactly the same, but the locality would be shifted; this crucial test proving the fault to be in the screw.

That lines cannot be ruled fine enough upon glass, but can be etched by the fumes of hydrofluoric acid, is a statement which is somewhat curious, because the fumes etch laterally—i. e., not only straight through the glass which has been exposed, but underneath the protecting wax on either side of the line—thus widening the

line as much on each side as the depth of the etching; secondly, because, independently of the peculiarity of the etching, the lines were first of all ruled by the act of clearing away the wax coating to expose the glass to the action of the fumes; and thirdly, because the line thus ruled must of necessity be very much finer than it becomes when etched.

The present writer publicly thanks Dr. J. J. Woodward for his handsome present, in 1874, of wonderfully clear photographic prints of the first seven bands of a Nobert's plate, of a Lord's Prayer blackened in and fixed down with balsam, and a Lord's Prayer mounted dry and unblacked, in which the letters were less than the twenty-eight millionth part of an inch. The Quekett Microscopical Club also received similar photographs. A comparison of the three photographs affords visible proofs of the accuracy of the theory that the spurious lines alluded to by Mr. Rogers and by many other writers, are due to polarization alone, because, in the photograph of the Lord's Prayer, blackened in, and taken with a Powell and Lealand's immersion eighth, the surface of the glass and the black letters were equally and only in focus at one and the same time, thereby producing a clearly defined Lord's Prayer *without any "spurious lines,"* because there was no possibility of producing any other than the normal refraction upon the entrance of the rays; while the photographs of the unblacked Lord's Prayer and of Nobert's lines both gave spurious lines, evidently from the same cause, but under different aspects, inasmuch as the Lord's Prayer is equally cut throughout, and therefore all in one focal plane, while the lines of Nobert vary in depth, the one band from another, thus precluding the possibility of each band being in focus at the same time—e. g., when the seventh band is in focus, being at the surface of the glass, the first band, being much deeper cut, is out of focus, except at the very edge of each side of every line, giving a broad white line, having on each side of it a broad densely black line, the true line being white in consequence of the $\frac{1}{8}$th being incapable of penetrating the depth of the cut beyond the mere entrance, and leaving in the focal plane a space of the cut wholly without substance. The intangible black lines, although very palpable to the mind, are easily demonstrated to be due to polarization, if a gentleman will use his polariscope with a clean plain glass slip upon the stage, when it will be found that at one period of the revolution of the polarizer the glass will transmit only rays of light closely resembling the invisible rays at the end of the solar spectrum, except that they are not violet but black, and are precisely the same as in the photograph, and produced by the same means, only altering the position of the prism and placing it (one to every line) by means of the V cut in the body of the slip; *each prism thus placed polarizing the light by again bending*

the rays already bent upon entering the body of the slide, and bending them to the right and to the left of a line.

Very great pleasure is felt in being able to agree with Mr. Rogers "that light is 'of too coarse a nature' to enable us to see particles of matter as small as $\frac{1}{200000}$ of an inch, is a conclusion which can be refuted without the slightest difficulty," for Mr. Crisp's Lord's Prayer, with the letters $\frac{1}{2100000000}$ of an inch is in itself a handy and indisputable refutation, if the $\frac{1}{2100000000}$ of an inch be analyzed, brought down to its smallest fraction of the contorted intersected line forming the letter, and then reduced to its square root.

IV.—*On the Present Limits of Vision.*

By Dr. Royston-Pigott, M.A., F.C.P.S., M.R.C.P., F.R.S., F.R.A.S.

(*Taken as read before the* Royal Microscopical Society, 1876.)

The subject of minute vision, and the limits to minute investigation, assigned to the nature of light, to the powers of the organs of vision, and to the construction of optical instruments, is full of a deep and abiding interest. To this end, the powers of philosophers in all ages have been anxiously turned. The subject is doubtless one of extreme difficulty and delicacy. I must frankly own, that I believe this limit has not yet been reached. The reasons which may be given for this belief, may be conveniently arranged under the nature of light, the powers of the eye, and of the instrumentation.

1. *The Nature of Light—Vibration and Colour.*

If light consists of a vibrating medium, forming waves, it is argued that the wave principle necessarily precludes, under the most favourable conditions, the formation of a visible image less than half a wave-length. This theory, originally applied by La Grange, has been popularized by Helmholtz and Abbé. Sir Richard Airy, in his undulatory theory, shows that a minute disk of light assumes a spurious diameter in proportion to the smallness of the aperture through which it is viewed. If, for instance, a brilliant luminous point be examined through a pin-hole perforated in a card, the disk of light is enlarged apparently in proportion as the aperture is reduced. In the same way in telescopes the disk of a star, always enormously spurious, is diminished as the linear aperture is increased; whether the instrument be a reflector or refractor. And in the microscope the same thing is accomplished by diminishing the aperture by a stop placed immediately behind a compound achromatic objective. And various rings should be developed around the spurious disk in every case.

These are observed facts, nicely agreeing with calculated results. All these results may be combined in one conclusion—the effective linear aperture of the pencil of rays presented to the eye of the observer by whatever instrumentation regulates the apparent diameter of the spurious disk and companion diffraction rings, while other circumstances modify their appearance, form, number, and prismatic tints, or even cause their obliteration.

But in order to develope the spurious disk and companion rings, a brilliant point of light is required. These phenomena, observed by means of the microscope, are very similar to those seen

with the telescope. Now whether the point of light is viewed or a series of points forming a continuous line, as a polished rod of metal or glass in sunshine, diffraction effects are equally produced. And as a rule few eyes are capable of separating double disks or lines of light whose dividing interval is less than a minute of arc. But if the disks are obtained by solar light, the instant the sunshine is obscured by a white cloud, fine details much more minute than the double disks or bright lines become plainly visible: the diffraction had thus obscured objects much smaller than the fringes—a most significant fact.

Example. Suppose a highly finished brass instrument is placed at a distance and viewed with a high power by an excellent telescope. At several points in bright sunshine brilliant disks and lines of light surrounded by a black border and continuous diffraction lines will be readily seen. A variety of objects placed together, as black glass beads, thermometer bulbs, and other shining surfaces, give by comparison numerous diffraction appearances; two polished needles placed very close together, reflecting threads of light in sunshine, become blended and invisible from these diffractions. But the moment the sun goes behind a cloud, the details become instantly visible. Precisely the same effects are produced by forming miniatures of such objects in sunshine and then viewing these miniatures with a high-power microscope. For this purpose gold leaf pressed between a glass cover and slide displays beautiful fringes, and especially the black interference ring, under transmitted solar illumination.

Supposing then that the eye of the observer is so keen as to be able to separate two lines of bright light subtending a visual angle of one minute in the field of view, the instant the illumination of sunshine ceases, minute details of the objects become visible which had been totally hidden by the spurious disks and enlargements of brilliant diffractions.

Experiment 1.—An optical gauge containing seven standard double convex lenses having their focal lengths respectively

$$1 \text{ inch}, \ \tfrac{3}{4}, \ \tfrac{1}{2}, \ \tfrac{1}{3}, \ \tfrac{1}{4}, \ \tfrac{1}{5}, \ \tfrac{1}{6},$$

was placed in sunshine, and in such a position between the telescope and the house either that brilliant disks could be observed by reflected images of the sun, or under passing clouds the beautiful details of the miniature house* of a very light colour could equally be examined. During bright sunshine nothing but the spurious disk and companion rings of diffraction could be descried.

* This object, as being white and relieved with dark-looking windows, chimneys, and a background of lofty trees, gave a beautifully minute picture, displayed by the half-inch lens about 60 feet distant.

The miniature landscape disappeared. But in shadow very minute details of the miniature house came sharply into view.

Experiment 2.—A mercurial thermometer bulb about three-quarters of an inch in diameter was placed at fifty yards distance. In shadow a very fine telescope enabled me to descry the miniature of trees and the chimneys of the house against the sky. In sunshine the diffraction hid all these details.

If a long line of light be similarly observed, such as a brightly reflecting steel rod, the diffractions follow accurately the outline of the rod, and elegantly turn round the ends with their exquisite diffraction rings beautifully defined according to the quality and power of the telescope.

The $\frac{1}{4}$ inch lens appeared to give the neatest disk; the larger lenses gave too large an image of the sun, and spoiled the diffraction phenomena. Still better than reflected light is transmitted. For when a prism is so placed as to give total internal reflexion, the miniature of the sun assumes a surpassing splendour, appearing like a veritable electric light of overpowering brilliance. A lens of $\frac{1}{4}$ inch focal length then gives a solar disk of $\frac{1}{400}$ of an inch nearly in diameter $\left(\frac{1}{4} \sin. 30' \text{ nearly} = \frac{1}{4} \times \frac{30}{3438}, \text{ since } 1' \text{ nearly} = \frac{1}{3438}\right)$. But when even a lens of $\frac{1}{20}$ is used, the disk is the same size, only less bright. In that case the size of the disk is

$$\frac{1}{20} \sin. 30' = \frac{1}{20} \cdot \frac{30}{3438} \text{ nearly} = \frac{1}{\frac{2}{3} \times 3438} = \frac{1}{2292},$$

or the $\frac{1}{2000}$ of an inch nearly. Now this placed at fifty yards would be $0''\cdot0573$, or $\frac{1}{17}$ of a second, impossible to be seen by any telescope in existence. This again shows the tremendous expansion of a point of light, being an exact miniature of the sun $\frac{1}{2000}$ in diameter. For it appeared quite a considerable disk, fully one-sixth of an inch in diameter, or at least three hundred times its proper size.

The optical instrumentation by which these disks are presented to the eye by the eye-piece may be of many kinds—a long tube, as in the telescope, or a short one, as in the microscope: the effect is precisely the same for all. The eye indeed may be covered with a minute aperture, and look directly (but nearly) at a brilliant point of light: in each case the disk is expanded into considerable dimensions with its accompanying rings. In using these disks with the microscope, the instrument is set horizontally, so that the $\frac{1}{8}$th object-glass used as a condenser forms a minute brilliant miniature disk at 100 inches distance. My immersion $\frac{1}{8}$th is really $\frac{1}{10}$th, and the image is diminished one thousand times; the theoretical image of

the spurious disk ought to be then one thousand times less than the $\frac{1}{400}$ of an inch, or $\frac{1}{400000}$. But when accurately measured by a very excellent micrometer by Browning, it measured $\frac{1}{16000}$ nearly; therefore the disk was enlarged by diffraction to twenty-five times its correct size. This result shows how hopeless it is to measure under the microscope the size of unknown brilliant objects illuminated with intense sunlight. With all the advantage of the widest angular aperture, which, as we have seen, diminishes the spurious disk proportionally, an aperture of 150° giving a much smaller disk than one of 70°, still the brilliant disk was at least twenty times too large, and this too with the finest glasses extant.

The telescope or microscope properly handled give just the same evidence in kind. The latter instrument gives far the most startling results as to the limitation of vision when brilliant points are allowed to develope all their wonderful diffraction phenomena, but both instruments can be alternately employed for the purpose of illustrating the present limits of vision, remembering, however, that the best microscopes give very much smaller diffraction disks and rings than the very best telescopes, power for power.

There can be little doubt that much of the clever manipulation of the olden times with inferior glasses owed its success to the patient and experienced, yet unconscious reduction of injurious diffraction; as well by modifying the light as its direction and kind. In former times, the precise position of the mirror for throwing the rays of reflected light at one particular angle (often hit only with much waste of time and labour) was attained with more or less success, so as to give the most brilliant definition of difficult objects. In 1862 I adapted a semicircular arc carrying a condenser, and afterwards I constructed gimbals to carry an achromatic condenser at any angle of obliquity, attached to a double-motion stage placed exactly beneath the upper stage movements: by this contrivance particular angles of illumination could be readily attained without the excessive aberration of the usual wide-angled achromatic condenser. The instrument is exhibited in the South Kensington Museum Collection, No. 3551. Described as follows:—

"3551. *Microscope* with complex adjustments, searcher, and oblique condenser apparatus. Dr. Royston-Pigott, F.R.S.

"This microscope is fitted with a peculiar hypocycloidal movement and traversing screws for very delicate observations. The condenser possesses wide rectangular movements combined with a unique oscillating oblique action for directing the minute image of a flame or of the sun either directly or obliquely upon any desired point in the field of view, giving fine views of many difficult objects, and gorgeous diffraction phenomena with circular solar

spectra. It is also fitted with Dr. Royston-Pigott's searcher for aplanatic images, by which much greater depth of focus is attained, and new powers of correcting chromatic and spherical aberration, by moving the searcher between the objective and eye-piece."

If then it can be clearly shown that brilliant points and lines are immensely enlarged by diffraction (as indeed calculated from the principles of the theory of light), and that the fringes thus developed by its undulating waves can be calculated so as to ascertain the conditions of effacements of contiguous images, it certainly follows that the limits of vision can be assigned for brilliant points and lines. But the instant the brilliant illumination ceases, these spurious disks vanish, and also the peculiar fringes of diffraction. An excellent example of this is seen in a fine telescope. When a double star, such as ϵ Boötes, is observed in bright twilight, I see then nothing but two pale snow-like disks on a milky blue sky: without the slightest appearance of diffraction rings. On a dark night the smaller star appears to fall upon a bright diffraction ring, which greatly obscures its appearance and the former wide space between the companion and its principal. The white cloud illumination, especially from a north sky, has long been recognized as a marvellous improver of definition, for the very same reason—*reduction of diffraction.*

These points of brilliant light are immensely enlarged into spurious disks even in the best telescopes (and therefore in microscopes, as often demonstrated by the writer). I may give a most decisive proof of this, as follows:—I found in an excellent Wray telescope, of $5\frac{1}{4}$ inches aperture and 8 feet focal length, the disks of ξ Ursæ Majoris in May last just in contact. These stars are suns of nearly equal magnitude, separated from each other by probably a thousand million of miles at the least; yet their apparent disks were so much enlarged as to appear almost to touch each other, a thin dark line separating them. Their centres are separated by a little more than two semi-diameters, and these are nearly equal. Hence the diameters of these suns appear at least 800 millions, showing how enormously enlarged are the apparent spurious disks in my telescope above the true size. The Rev. T. W. Webb writes me that with his very fine $9\frac{1}{4}$ "With" mirror, these stars appear about half a second apart.

In a question of so general an interest as the powers of vision, the labours of the Jury of the Great Exhibition of 1851, in determining microscopic power, are highly instructive. Nobert's celebrated lines 11,000 to the inch were shown with a linear amplitude of 100, but lines 50,000 to the inch required a power of 2000; that is, lines four and a half times closer required twenty times the power. This strange disproportion of power to size

excited much attention.* So many persons in recent years have seen Nobert's bands at least twice as close with half the power, that the conclusion can hardly be avoided that the glasses must have been of late greatly improved in quality. The opticians, by the continual study of these lines and of the natural lines of the diatoms (formerly so much admired, before a more perfect resolution had been attained), at length drew forth the practical result that the widest possible aperture was absolutely required for the highest efforts of resolution in these interesting objects. This practical result anticipated by many years the theory lately popularized by Helmholtz.

According to the results of the undulatory theory of light, the size of the fringes of diffraction of a bright disk or line of light which are capable of totally obscuring an object of less diameter than these fringes, varies as the sine of the half aperture for the same wave of light. Accordingly the resolving power for brilliant disks or lines of light varies proportionally to the natural sines of these apertures for one kind of light. For †

$$\epsilon = \frac{\lambda}{2 \sin. \alpha}.$$

If λ be constant, ϵ varies inversely, as sin. α or sin. semi-aperture.

Professor Helmholtz having given some prominence to this formula, deduced from those of La Grange, it may be well to repeat that—

ϵ represents the smallest interspace recognizable between two bright lines or disks: on the condition that the diffraction fringe of one does not overlap that of its neighbour.

λ represents the length of the wave of light under consideration, which for mean rays is generally taken thus:

$$\begin{aligned} \lambda &= 0 \cdot 00055 \text{ mm.} \\ &= 0 \cdot 00055 \times \cdot 0393708, \end{aligned}$$

the metre being 39·37078984 English inches, so that I find

$$\lambda = \tfrac{1}{46182} \text{ inch.}$$

And half the wave-length for an extreme aperture of nearly 180° is therefore $\frac{1}{92364}$ of an inch. This very curiously closely approximates to the recent elaborate measures of diatoms, such as the *Amphipleura pellucida.*

ARGUMENT: ϵ VARIES as $\dfrac{1}{\sin. \alpha}$, $\dfrac{\lambda}{2}$ BEING A CONSTANT FACTOR.

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* I am indebted to Mr. Broun's paper for this statement.

† Dr. Fripp has done great service to the readers of the 'Monthly Microscopical Journal' by his able translation of the paper by Herr Helmholtz, one of the most brilliant of Continental mathematicians.

TABLE OF PROPORTIONATE RESOLVING POWERS.

Full Aperture of Object-glass.	Proportionate Resolving Power.	Semi-aperture a.
179°	sin. a = 99996	89½°
175	sin. a = 99905	87½
150	sin. a = 96590	75
120	sin. a = 86600	60
100	sin. a = 76604	50
80	sin. a = 64278	40
75	sin. a = 60870	37½
70	sin. a = 57360	35
65	sin. a = 53750	32½
60	sin. a = 50000	30
50	sin. a = 42260	25
40	sin. a = 34200	20
30	sin. a = 25880	15
20	sin. a = 17360	10
15	sin. a = 13050	7½
12° 38′	sin. a = 11000	6° 19′

One remarkable result of this table is that if 96,000 brilliant lines can be resolved with an aperture of 150°, then 11,000 lines per inch ought to be resolved with so low an aperture as 12° 38′. I should remark that the numbers given in this table are proportionate numbers, and not absolute, but they nearly correspond to the results of delicate observations.

But with special adaptations to subdue or destroy the brilliant diffractions of too bright an illumination, many minute details before completely effaced may be brought into distinct revelation.

Photographs taken with brilliant sunlight transmitted through transparent objects capable of forming brilliant lines of light and lens-like images of the sun, are necessarily subject to brilliant diffractions; bright lens-like images are considerably enlarged, and look like blebs, swellings, and grotesque protuberances. At the same time, under proper precautions, and the use of light of the smallest available wave, these diffractions can be reduced. Besides this, the choice of object-glasses, however good they may be for other work, is important. Some of these producing very much less diffraction than others; some giving fine and others coarse diffraction lines.

2. *The Powers of the Eye, and Instrumentation.*

Acuteness of vision varies so considerably in different individuals as to render an average estimate somewhat difficult. I witnessed the accidental detection of Jupiter's satellites (three mentioned) by a person unacquainted with their existence, and this person at my request drew their position on paper, which exactly

corresponded with my view of them through a good telescope. Another individual distinguished two children ascending the sunny side of a hill, and the colour of their jackets, at a distance exceeding half a mile (also verified with a good opera-glass). The same person could see bullet marks at 500 yards. Another fact was very surprising. I watched from the Ramsgate sands, for a long time, in 1844, a balloon (which had gone off towards Holland) with a small opera-glass magnifying about $2\frac{1}{2}$ times. Long after it ceased to be visible to me with this aid, the sailors lounging about kept watching it still, and several saw it distinctly with the naked eye.

Another circumstance is worthy of note. In some persons striations or rows of beads can only be seen when presented to the eye at a certain angle. I recollect every one of a party of gentlemen at my house, except one, saw distinctly a microscopic field of this nature. I then said to him jocularly, "Turn your head on one side," when to his surprise the definition became quite distinct. I have often observed highly skilled opticians perform the very same gyrations.

Mr. Broun, F.R.S., says * (by error Colonel Woodward spells it Brown): A dark brown hair ·0026 inch wide, 2·5 inches long, was fixed by dots of transparent gum-arabic to the window-pane, and was seen by a young eye against a N.W. sky at 36 feet distance; the diameter of the hair subtended an angle of 1″·24 ($1\frac{1}{4}$ seconds of arc). Mr. Broun required it to be placed at 30 feet distance, and this would give a visual angle of 1″·54, a quarter of a second greater.

It may be interesting to the reader to know that a white disk of paper one inch in diameter forms a visual angle of

1″ at 206265	inches	distance, or	5730	yards.
2″ at 103132	"	"	2865	"
3″ at 68755	"	"	1910	"
6″ at 34378	"	"	955	"
60″ at 3438	"	"	95	"

Now a visual angle of two seconds is equivalent to

A line $\frac{1}{103132}$ inch diameter, distant 1 inch.
A line $\frac{1}{10313}$ " " " 10 inches.

In agreement with this, Mr. Broun states a young eye, he finds, can actually see lines on glass $\frac{1}{10000}$ inch wide, $\frac{1}{28}$ long.

If therefore the 10,000th of an inch can be seen with the naked eye, without a lens, it ought to follow that the 100,000th of an inch ought to be seen by the same acute eye, with a power magnifying ten times.

Now Nobert's lines 112,000 to the inch, probably have inter-

* 'Proceedings Royal Soc.' p. 523, vol. xxiii.

spaces quite as wide as the thickness of the lines (indeed, Mr. Broun's examination of photographs of these lines, as well as my own, confirm this estimate); and therefore the absolute diameter of the lines themselves would be about the 224,000th of an inch. Such a line, or rather, if conceivable, such a black line as this would, if placed at ten inches distance, subtend an angle of

$\frac{1}{9}''$ very nearly (one-ninth of a second).

Viewed with a power of 18, its angle would be

2 seconds.

With a power of 540 the visual angle would be raised to

$60''$ or $1'$

(more accurately $\frac{1}{9 \cdot 2}$ second, which would give 21·75 instead of 18, and then the power would be 650 instead of 540).

If therefore the lines on Nobert's plate 112,000 to the inch were really simple black lines, they ought, with ordinary sight, to be easily distinguishable with a magnifying power of about 600 diameters, and this would make a visual angle thirty times greater than Mr. Broun's result above stated.

But these lines in general are grooves ploughed in glass of a prismatic, round or irregular section; and since they can only be seen with extremely oblique illumination (looking as it were sideways) by means of the very wide-angled objective generally found necessary, it is probable that the available shadow may be much less than the supposed breadth of the line, and quite indeterminable.

In cutting lines on glass with a diamond, I have been occasionally much surprised with the beautiful little curls or ringlets cut cleanly out of the glass surface; but this only happened when the diamond holder was rotated into one particular position, and inclined at one particular angle. When, therefore, we are looking at such fine "Nobert" grooves in glass, we are somewhat in the dark as to what kind of object or shadow we are really observing. For if different grooves be cut in glass, forming differently shaped channels in section, whether oval, circular, square, or triangular, a remarkable difference in appearance will be observed when viewed and illuminated obliquely with transmitted light. Nobert's grooves are, as it were, unknown objects, for we know not and never shall know the sectional shape of the hollow rulings that compose them.

The detection of these lines is essentially fitted for high-angled glasses; but for real useful physiological work they give but an indifferent test of depth of focus, so requisite for everything but mere surface markings. When we descry Nobert's XIX. band of lines, do we see the breadth of a line, the shadow of the side of a

groove, or a diffraction compounded of the shadows of contiguous lines?

It is to be regretted that this great artist in minute engraving does not make some of the lines longer than others, so that the behaviour of a single line produced could be carefully scrutinized, as suggested by Mr. Broun.

The limits of vision are also affected seriously *by the colours of bright objects,* which are of necessity developed by their position in different focal planes, and particularly by accidental destruction of diffraction.*

There are many persons who still strongly object to a variegated display of colouring in beaded objects; those who have been long accustomed to purely achromatic glasses, such as were attempted to be made as the highest pride of the celebrated Andrew Ross, who succeeded in producing glasses so nearly achromatic, that an ordinary object, such as the Formosum, appeared of a very pale yellowish tint, approaching white, relieved by dark markings or striæ. I recollect the *double* striæ of the Hippocampus were so displayed to me about thirty years ago by the famous "Topping" with a "quarter." And the attainment of this result with a quarter was regarded as a fine reward of manipulative skill. Well, if we compare these old glasses with the most splendid productions of the present day, the pale yellow and dark grey tinge have vanished in favour of a rosy pink and black. *This change is highly significant. Higher and deeper planes develope colours,* forming the colour test formerly described by me; whilst the finest focal plane presents black lines combined with a pale rose-pink blush.

Turn we again to the telescope. This instrument beautifully illustrates the optical qualities of the microscope. Let E O P Q R be the axis of the telescope directed to three brilliant minute disks, P, Q, R, so arranged that each disk can be seen in the same field of view.

' E ———— O ———————— ', P ——— Q ——— R

(1) Let the telescope be focussed on the middle disk Q, then, if the telescope be of the very finest quality, the disk Q will appear white; but the disks placed within and without the focus will at the same time appear of totally different colours, according to their distance from Q. (This is a novel test of the corrections of the glasses.)

(2) Focus the telescope on R, the most distant point, then the disks Q and P will totally change their colours, according to their distance from P.

* "It is not impossible," says Helmholtz, "that by some fortuitous overlapping of images, objects of still smaller dimensions might occasionally be seen," i. e. less than half a wave-length.

(3) Focus upon P, then the colours of Q and R will appear totally changed.

Collecting these facts, it will be found that the colours of each of the three disks P, Q, R, do change their colours incessantly with every slight alteration of focussing.

A most brilliant blood red, rose colour, lavender, purple and yellow green, may be readily obtained by mere change of focus: and if the number of disks be increased, and their distances varied, a gorgeous variety of colouring will be displayed, eclipsing the richest tints of the rainbow: provided sunlit disks be employed. The telescope, which is of the finest quality, will show the greatest variety of beauty in these coloured disks, one only—namely, that which is in the most nicely adjusted focus—appearing white.

It may also be remarked that the quality of the telescope may be determined by the *smallness* of the change of focus, which produces a coloured disk from a white one; and the rapidity with which the disk goes in and out of focus. It is utterly impossible, in the nature of light and achromatism, that brilliant objects in different planes of vision, at a greater or less distance from the eye, can be all in focus at once. Those that are nearer the eye than the true focal point will be of a different hue from those farther from the eye and beyond the focal point. If the telescope be under-corrected, the nearer disk appears ruby red and the farther a deep blue: with all manner of intermediate colours for intermediate brilliant points. The same holds exactly true for the microscope as for the telescope, only it is produced upon a very minute scale of focal distance.

Apply then these facts to the microscope. Suppose a closely beaded object is illuminated transparently, and that several masses of beads, though close, lie really in different focal planes: these beads, if refracting, become brilliant disks, though excessively minute. For a perfect objective, only one set can be in the same mathematical focal plane. Suppose one of these to be Q, then those lying nearer to the eye, as P, will necessarily appear of a different colour from those beyond Q, as R; and according to the variety of their situations, so must necessarily their colour vary to the eye of the observer.

There are times, as already remarked, when, owing to good fortune in the various conditions of vision, appearances are developed which are often again sought for in vain. A lucky turn of the screw-collar; or of change in the length of tube; thickness of covering glass, and the best kind and colour of illumination, may reveal to the glance of the microscopist novel structures not perhaps easily caught again. During the astonishing though gradual advances made in the powers of the microscope, most persons who have watched its history and career during the last

twenty-five years, must have been frequent witnesses of peculiarly evanescent forms, due to better or worse conditions of vision to which the instrument and the observer are occasionally subject. Diffractions of an unexpected character may reveal extraordinary improvement in defining power. If, for instance, a fine object-glass be used as a condenser, and the image of a gas flame lie in the field of view, perhaps at the edge of the flame, or at the edge of a piece of bright brasswork in the image, may be detected at once what had only been previously accomplished by long and patient labours. It can never be forgotten that all rays passing from a brilliant object up the microscope are not equally useful or equally potent in forming the correct image. The great art of perfecting definition from a given instrument often consists in destroying the useless and preserving the real working rays. General opinion seems gradually to have come round in favour of pin-hole stops for distinct definition; the effect of which is to limit the illuminating rays and prevent the object being drowned in excess of light. A pin-hole stop limits the illuminating pencil to perhaps ten degrees. But this stop is often in most dangerous proximity under the slide; and the object may be pinched between it and the nose of the objective, and cause destruction of both. An inch and a half objective * is generally of the same aperture (ten degrees), consequently the very same kind of illuminating pencil can be obtained from this as from the pin-hole without this source of annoyance: in addition to this, a Beck iris diaphragm attached below this kind of condenser gives every degree of fineness required in the illuminating pencil. Great advantage is also sometimes obtained by stopping off half the rays.

In some cases of difficult beading, whether of scales or diatoms, I have found a U-shaped aperture highly advantageous. When a condenser is used with its axis obliquely inclined, some difficulty may be experienced in centering the lights. To obviate this, I have found a piece of transparently red paper gummed on to the front lens quite effectual.† The light shows itself through the same opaque paper, and informs the observer of the exact method of getting nearly the whole U aperture filled with the illuminating pencil. Very fine effects may sometimes be produced by a 4-inch opera-glass (used as a condenser), the eye-lens being removed: still finer, if such a glass be used to give parallel rays instead of the bull's-eye: the lamp flame being placed in its focus of course discharges a column of parallel rays upon the object. In proportion, however, as the rays become more nearly parallel, the light, if artificial, may be too much weakened. On this account a pencil of 10° or even 15° is preferred, such as is given by a low-power objective. I have found in several investigations aplanatism in condensers quite as necessary

* A pin-hole stop has about 10° of aperture.

† Out of which a U-shaped aperture has been cut. Blue paper is better.

as achromatism for illuminating lenses. In giving directions for the detection of Podura beading six years ago, particular allusion was made to this; and in the present improved state of object-glasses, much freed from spherical aberration, as shown by the more recent efforts of Messrs. Powell and Lealand, Wenham, and others, I may be permitted to again draw the attention of the Society to what may still be considered interesting points, and by no means exhausted. I said: * —

"The extreme difficulty of defining a minute row of beads arises from the uncorrected aberrations confusing their images, by which several images overlap and obliterate the form of individual beads. . . . . In my experience, I have found an oblique centrical pencil of achromatic cones of light of small aperture (15° or 20°) of the greatest practical utility, the obliquity being varied according to the object in view.

"Another circumstance worthy of note, viz. the position of the stigmatic image, or of the distance from the back lens of the object-glass, where there is a *real* focus. . . . . A search for the real focus or best image should not be neglected along the axis of the instrument." So far in 1869.

According to my own investigations, there are many objectives which refuse to give their best defining powers at the particular rigid distance of ten inches. How much easier to alter the length of tube than the intrinsic corrections of the glasses! A telescope tube, or one built up of pieces, is one of the simplest of all adjustments for over or under-correction. I prefer to use my 50th objective with half the ordinary length. It gives a longer focus, allows a thicker covering glass, and displays a brilliance of detail in this way sometimes quite amazing. By shortening or lengthening the eye tube, the observer is actually making, perhaps unwittingly, a most philosophical search for a real aplanatic focus. Very few people know that it is the last back lens which makes the last final image within the tube at the focus of the eye-piece. Each lens makes its own, which in its turn is taken up by the next, and so on. Some lenses have one, some two aplanatic foci—i. e., at certain points only in the tube. Find them by all means, if you can, by thus searching the axis. Perhaps you will get a magnificent effect at one length, impossible with another. The screw-collar does a good deal, but not all that is required to open out latent beauties perhaps hitherto unseen.

Another point bearing on the subject of this paper is the use of the searcher for aplanatic images. A lengthening or shortening tube is one of the best of searchers. Combined with this is the subtle effect of moving to and fro lenses between the eye-piece and objective.

* 'Monthly Mic. Jour.' Dec. 1, 1869. Sent to Society, May 21, 1869.

In the 'Quarterly Journal of Microscopical Science' some one has lately said Dr. Pigott has given up the use of the searcher; a more silly and gratuitous statement could not well be made by an anonymous writer. On the contrary, I am trying very much to improve and simplify it. I have adapted one especially for a Gundlach. It is a 16th (English 13th). My searcher deepens its focal distance, enables me to use low eye-pieces instead of deep ones (which I hate). I get jet-black edgings with scarcely any burr of light. I have seen as well with this combination as with any glass I have ever tried. I can vary all the corrections at will, far beyond the limits of mere "collar" action. I change the colouring; blacken the shadows; embrilliant the bright focal points of isolated refracting particles; evanish the focal plane more suddenly. In fact, as soon as the corrections become as perfect as the combination admits, *less light is required.* Wasted light is concentrated. Ridge after ridge glows into focus as each flashes into the focal plane of vision. A rolling line of brilliance, the locus of the most exact definition, moves backwards and forwards along a curved surface, with the slightest touch of the fine focus wheel. The object, in a manner, becomes as it were self-illuminated. And in proportion as less intrinsic light is employed at the origin of light, the diffraction errors are correspondingly reduced, and may be in some cases destroyed.

I may be permitted to illustrate these points by actual cases.

I. A gentleman and his wife on a visit to my house made the following note in his own handwriting, and I had nothing whatever to do with the description or diction; only with the adjustments and illumination.

"*Degeeria domestica.*

"Paraffin lamp, direct light illumination, $1\frac{1}{2}$ inch open condenser.* Gundlach's $\frac{1}{16}$ with new correcting lenses and A eye-piece: 6 inches tube: blue glass shade.

"*Glass under-corrected to show upper surface.*

"(1) *At lowest focus:* i. e., focus for lowest surface. Rose colour on *green ground.* Longitudinal beads and 'bugles,' or short tubes.

"(2) *Slightly higher focus. White ground.* 'Tubes' dark purple with light green, and pink edges; indistinct pale spaces between the 'tubes.'

"(3) *Slightly higher focus.* Very faint 'beads' transverse between the 'tubes'; *ground variegated in colour;* 'beads' not separated, apparently joined; tubes pale green in colour. (Recorrected by screw-collar.)

"*At upper focus.* Scale broken, and in part obliterated. Ground yellowish green. Clots of longitudinal beads, bright or blood red.

* Without U-shaped stop or any stop at all.

Eight beads were seen to run together and form three beads. Twenty beads counted in one row. Between the rows of red beads *very small dots visible of dark colour.* The terminal beads of each row are emerald green.

"*Focus slightly lower.* Beads green, *with dark round ring round each.*

"*At upper focus.* On removal of blue glass shade beads are yellowish red; ground orange colour.

"*Description of the Quill.*

"*At lowest focus.* Centre, i. e. tube of quill green, with black border. White halo.

"*Focus slightly higher.* Centre paler green; border line quite black; halo tinged with pink.

"*Focus slightly higher.* Centre pink; border line deepest black, and edged with light green.

"*Focus slightly higher.* Black lines very sharply defined. Centre (of quill) pink beads, eleven or twelve counted.

"*Podura domestica.* Under same conditions as on preceding day. Scale unbroken. (A different scale chosen.)

"1. *At highest focus.* Longitudinal lines of red beads. Beads not all of same size.* Ground yellowish green. Some of the beads tipped black in centre. The edge of the scale is not a continuous membrane, but resembles a twisted rope of the same colour as the beads in the interior. The beads are strung together irregularly —i. e. the numbers in each string varying.

"2. *At middle focus.* Two distinct sets of beads longitudinal and transverse. Beads are of different colour, some dark green, light pink, light green, and black: all the beads strung in one group are not of the same colour: rouleaux of green, red, and black beads principally distributed longitudinally. The form of the rouleaux is generally wavy."

The most remarkable points in these observations made by my visitors (one acting as amanuensis) are the beads seen tipped with black, the deep black border line, and the dozen pink beads within the quill, as also the variation of colours in different focal planes.

One other thing is worthy of note. "*Between the rows of red beads very small dots were visible of a dark colour;" and the terminal beads of each row were emerald green.*

I need hardly say this perfection of definition is difficult of attainment. The most perfect form of definition I have witnessed, and still more baffling, is to observe six minute dark beads filling up

* Mr. Browning first pointed out this peculiarity to me at my house in March, 1869, and alluded to by him in the discussion on the Podura paper same year.

the interstices of the large beads, closely packed and nearly black, estimated at about 130,000 to the inch.

I venture the prediction that the glories of the Podura scale will not be exhausted for many a long year to come. I await with hope the time when these apparent limits of vision shall be greatly surpassed.

The same goodness of definition was displayed in the "dry mount" Formosum, the spaces between the spherules occupied by a different set: individual beads appear bright in the centre, surrounded by a very dark ring, and show a very perceptible bright focal point. A fine state of defining power appears to light up as it were the usually dark dull appearance of this dry object.

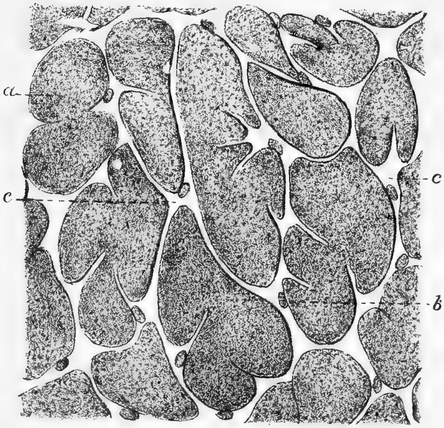
The Rev. Mr. Dallinger's Plates of the flagella of certain monads developed in the decomposition of cod's head, render it probable that he has been able to distinguish threads of a much less diameter than the $\frac{1}{150000}$ of an inch, with a Powell and Lealand $\frac{1}{50}$th, and very special precautions of illumination (see Plates to his article).

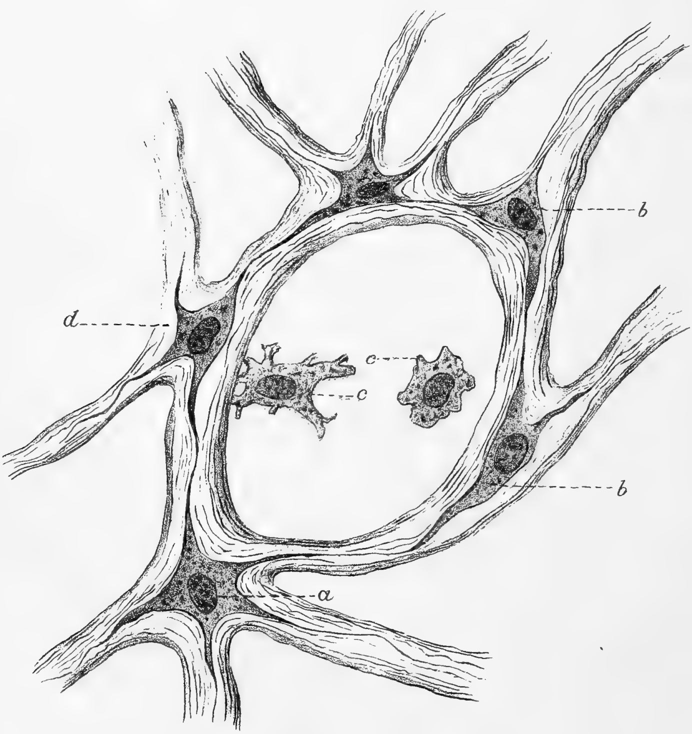
The apparent size too of the smallest cilia in a variety of organic structures, evidently demonstrates that a much less diameter than the diffraction limit as given by Helmholtz for brilliant images is often reached by the practised microscopist.

On the whole, I am led to conclude that although theoretically, and for brilliant lines and points, the separable interval may be for the widest aperture HALF A WAVE-LENGTH, yet when by proper precautions of illumination the diffraction can in a great measure be destroyed, with very accurate glasses the limit is much smaller than the one assigned. Even with the purple or rather the violet ray, the half wave-length for brilliant diffractions would be about $\frac{1}{150000}$ of an inch, but for cases of reduced or destroyed diffraction very much less.

Whether, then, the nature of light, as producing spurious disks and rings, or developing colours in different points of the focal axis, or the powers of the eye be considered, I have reasons to believe that the limits of vision have not yet been reached.

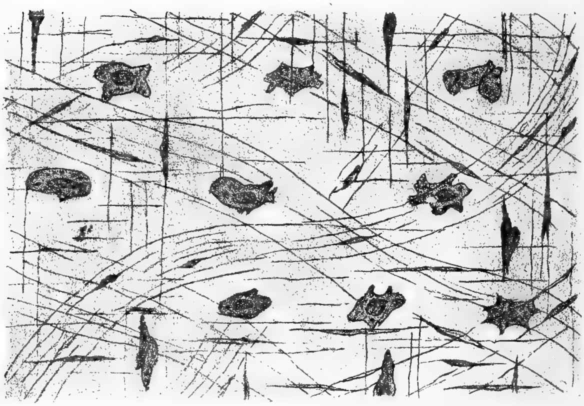
The diameter of the ocular pencil, indeed, for brilliant objects, regulates the size of the fringes inversely, which may efface contiguous images, but nevertheless, by means of small pencils, under special illumination, the eye is capable of descrying objects much smaller than the effacing fringes when skilfully reduced, if not destroyed. Manipulative skill, therefore, is still a great desideratum for the microscopical tyro who aspires to pass beyond present attainments.

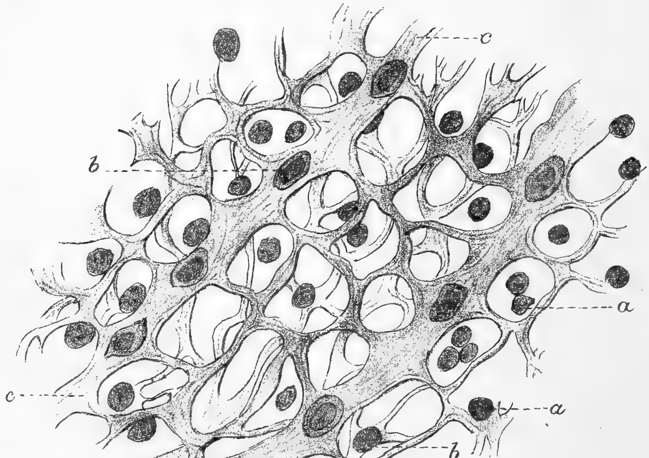




W. West & Co. lith.

Structure of connective substances





W. West & Co. lith

Structure of connective substances

V.—*On the Structure and Development of Connective Substances.*

By Thomas E. Satterthwaite, M.D., Microscopist to St. Luke's Hospital (N.Y., U.S.A.).

Plates CLVI. and CLVII.

During the last few years a great deal of attention and study has been directed toward a somewhat remarkable group of substances that enter largely into the composition of the body, where they form, in union with one another, a connected system, and so have obtained for themselves the name *connective substances*, their office being to furnish support or protection for the vessels, nerves, muscles, or epithelial bodies.

One or more of these substances may be found in every tissue or organ, where they are deeply concerned in all changes, such as those of repair, degeneration, or disease. The evident importance of knowing the characters of these substances in their various modifications has attracted to them a great deal of study, but opinions are still somewhat divided about them, there being little definite agreement as to the structure they exhibit even in the healthy and adult condition.

It has therefore been difficult in all cases to decide how much the modified appearances they present have been the result of altered action, or merely variations that belong to the natural life of the substance.

The determination of what constitutes the normal condition is, then, a matter of the first importance, and it is in this direction that the present inquiries have been made. The present essay is designed to embody a series of experimental researches upon the general subject of *connective substances*, with the view of determining some of the more important facts that have been matters of controversy.

The name *connective substances* has been adopted because it is already in use by leading histologists, and because it is an extremely convenient word under which to group together a large number of animal substances that have a very close relation with one another. The name was first proposed by Reichert in 1845, and embraces *bone, cartilage, dentine,* and the delicate forms called *mucous tissue, adenoid tissue, neuroglia, fat tissue, fibrillated connective tissue* (*fibrous tissue*), *intermuscular tissue, corneal tissue, tendon tissue,* and *elastic tissue.*

The general reasons for classifying these substances separately may be stated as follows: They are all said to be derived from the middle germinal layer of the embryo; * one form in one animal is

* The sustentacular tissue of the brain and cord is thought to be an exception to this. Frey's 'Histology,' p. 196.

often substituted for another form in another animal, so that fibrous tissue or cartilage in one animal will perform the same function as bone in another, and they have in this way come to be regarded as equivalents of one another in a morphological sense; again, it is quite apparent that one form often succeeds another form in the natural life of the body, fibrous tissue or cartillage of youth being transformed into bone in the adult. In the growths of tumours, these changes are frequently seen.

The word *connective tissue*, originally proposed by Johannes Müller, as distinguished from *connective substance*, has also sometimes been applied to one or more members of the same class, and, indeed, it is in this way that much confusion has been produced, for while some observers have used the word in the broad sense of connective substance, others have limited it to some specific form, such as fibrous tissue (fibrillated connective tissue).

To avoid any such source of error, we shall call each form by its distinctive name, as mucous tissue, adenoid tissue, and the like, and then we shall find that, though there is a strong bond of relation between all the forms, they (many of them) show as distinctive differences as any other tissues in the body. The word *connective tissue* will accordingly be avoided entirely except where its character is specifically described, as when using the expression "fibrillated connective tissue," or "connective tissue of the kidney," &c. We may then expect to get more precise notions of the minute structure of each variety, and so of the peculiar relations they each hold to pathological change.

The three that stand at the head of the list, viz. *bone, cartilage*, and *dentine*, are in many respects better understood than the others, chiefly because in gross appearance they show distinctive differences and because their anatomical elements have been more easy to isolate. The consideration of them, however, does not come within the range of the present work, and no further mention of them will therefore be made.

Our knowledge of connective substances dates from a comparatively recent period, for the first systematic efforts to determine their minute structure appear to have been made by Schwann in 1839. Since that time the doctrines in these matters have undergone important modifications, and it will be essential to consider the more important of them before we can get a clear conception of the views which are now entertained.

Schwann was the first to point out in these tissues certain bodies that he called spindle-shaped or caudate cells. The word "cell" is here used by Schwann in speaking of the variously shaped fixed cells, as distinguished on the one hand from the wandering cells which are now called leucocytes or lymphoid corpuscles, and on the other from the intercellular substance. The

word "cell" seems to have originated so much trouble that it would be desirable to avoid it entirely; but this is impossible from the very general use that is made of it. It will, however, be restricted in what follows to the fixed corpuscles of the parts. The difficulty in the use of the word "cell" has been, that observers have frequently, as we shall see, mistaken bundles of fibres for corpuscles, and because they are not agreed as to what properties belong to a cell. It has seemed better, therefore, to offer a description of appearances as they were noted during these studies from which we may subsequently decide whether or not they are to be called "cells."

It is proper to state that, previous to Schwann's discovery, it was supposed that all connective tissues, by which were meant *connective substances* in general, were made up of fibres, though even this had been denied by Reichert, who insisted that there were no fibres at all, but the apparent fibres were simply foldings of the substance. A new impetus to these questions was given by Virchow,* who at first opposed Reichert, maintaining that spindle-shaped or caudate cells did exist, and that in most cases the cells maintained their integrity, and consequently the connective tissue of early and late periods did not differ in general structure; the cells remained the same, though they were not so easy to detect. He further stated that the connective tissues (connective substances) could not be distinguished by the character of their cells, for in all connective tissue, round, angular, and long cells might occur; he also believed the cells were hollow and their processes hollow, constituting channels by which nutritive juices could be conveyed from place to place, being in fact like the lacunæ and the canaliculi of bone.

These views, however, he was obliged to modify at a later period.

Henle opposed Virchow's idea of connective-tissue corpuscles in certain particulars, especially in tendon tissue, and maintained that what seemed on cross-section to be cells, were merely spaces between the bundles, in which were nuclei and elastic fibres.† This statement was based upon a method he had of injecting the interspaces.

The figures that were regarded by Virchow as the stellate cells were, in reality, the angular spaces (Henle's spaces, Fig. 1, *c*) between three or more bundles, and they contained either a cross-section of an elastic fibre, or more probably, perhaps, the profile view of a connective-tissue corpuscle (*b*). As tendons contain but little elastic tissue, and the cross-sections of a fibre would be extremely small, the latter view is probably the correct one.

* 'Cellular Pathology,' 1871, pp. 69–73 and 131.
† Müller's 'Archiv,' 1852, p. 92.

Henle at an earlier period had described, as cells of this tissue, bodies that were like little plates, arranged in rows.* He undoubtedly was one of the first to get a correct conception of the real nature of these bodies. Later, Rollett also expressed somewhat similar views.†

Subsequently, great advances were made in these studies, at first by the use of acetic acid, which rendered the nuclei visible, and later, by the discovery of certain reagents which differentiated the elements even more strongly, and also by the application of certain fluids, such as Müller's fluid, which separated the bundles into their components, the fibrils.

Ranvier has really had most of the credit for directing the attention of histologists to the plate-like corpuscles, though, as we have seen, Henle had already mentioned them, and Ranvier himself credits him with their discovery.‡

Billroth also described them in 1858. Ranvier, however, gave the most distinct statement that had been made of the relations the corpuscles bore to the fibres. He said that these plate-like bodies formed a sort of investing sheath about the bundles, and so constituted hollow cylinders, something like drain pipes, the plate-like bodies themselves being held together by a firm, cementing substance. In some cases, however, these plates were not firmly united together in rows, but had considerable spaces between them, forming open or incomplete tubes. He stated, in fine, that connective tissue (by which, however, it is not clear exactly what varieties he meant it to include) was formed essentially of bundles of fibres, of elastic tissue, and of cells, and the bundles were cylindrical.

There were only two kinds of cells, one kind flat, containing granular protoplasm and nuclei, and having irregular outlines and prolongations; the other round and having nuclei, and not to be distinguished from white blood-globules.

Among the comparatively recent studies are those of Lœwe. This author has thrown a great deal of light upon the subject of tendons, especially upon their sheaths, which he believes are lined with endothelial cells, and constitute passages for the flow of lymph. He states also that the tendon bundles are covered with a continuous and closed sheath which is made up of the plate-like cells imbedded in an amorphous elastic ground substance, and that the bundles present the same characters for great distances.

These corpuscles, "Ranvier's cells," are also covered by another layer, which he calls the sub-endothelial layer, and which can be

* Canstatt's 'Jahresbericht,' 1851, p. 23.
† Henle and Pfeufer's 'Zeitschr.,' 1859.
‡ 'Archives de Phys.,' 1869, ii., p. 471.

distinctly demonstrated by what is known as the silver method.* The subject of these endothelial bodies is now attracting the attention of histologists, and promises valuable results. The views of later writers have so far agreed, that they have come to regard the fixed corpuscles of these substances not as spindle-shaped, but rather as thin, delicate, and plate-like. This view has, however, been attacked by Waldeyer as a generalization that has been carried too far. He believes that the corpuscles or so-called plate-like cells of tendon tissue and fibrous membranes are not simple, but complicated structures, and not single plates, but rather a number of plates meeting one another at different angles. The extremities of these plates terminate in fine processes that *often* anastomose with corresponding processes of other corpuscles; the nucleus is found on one of these plates. As for the corneal corpuscles, which have been so much discussed, he believes they are plate-like bodies, which are provided with distinct protoplasm about the nucleus, the amount diminishing towards the periphery, but in general characteristics do not differ much from the corpuscles of tendons and other fibrous tissues. The nuclei are difficult to make out, and are sometimes round, sometimes elongated like narrow rods, and sometimes are knobbed at each end, sometimes crescentic, and sometimes cruciform, though generally oval.†

It may be regarded as a fair statement of the case, if we say that most histologists believe that these tissues, generally, though we shall except from them elastic tissue, consist of certain fixed corpuscles of a plate-like form superimposed upon bundles of fibrils of indefinite length. As further exceptions to this may be mentioned, mucous tissue proper, in which there are no bundles; perhaps also, adenoid tissue, for it is said by Klein to be made up of netted cells, without bundles or fibres; so, too, it does not appear that the statement has ever been made that the neuroglia contains these peculiar bodies. The intermuscular tissue of the frog's thigh is also regarded by many as having no fibrils excepting those of elastic tissue.

Thus we see that, excepting only in the character of the corpuscles, there is not much agreement among observers, and even on this point there is difference of opinion.

It has seemed impossible to get a clear idea of these matters in any other way than by a systematic study of each and every one of the forms, subjecting them as nearly as possible to the same method of examination. This accordingly has been done, and the main inquiries have been directed toward—1. The general character and dimensions of the corpuscles in each; 2. Their relations to one another; 3. The character of the intercellular substance.

* 'Medicinische Jahrbücher,' iii. and iv., 1874.
† 'Archiv. f. mikrosk. Anotomie,' i., 1875, p. 176.

It is believed that the use of several new methods which do not appear to have been previously used in investigating the connective substances has helped to throw light upon these obscure subjects. The consideration of each tissue will be taken up in the order in which it has been tabulated. Some observations on *development of connective substances* will then follow.

1. *Mucous Tissue.*—It is well known that this substance is seen to great advantage in the umbilical cord of the embryo, and the following method has been found best suited to demonstrate it. Take a small piece of cord at about the third month and immerse it a few weeks in Müller's fluid; make a thin section through the very soft gelatinous part, then immerse it a few minutes in distilled water, to which subsequently a few drops of acetic acid are to be added, so that the solution shall not contain more than 1 per cent. of acid, and then mount in glycerine. It will then be seen that the softest portion contains numbers of irregularly shaped flattened plates, some containing an oval, flattened nucleus, others having none that are apparent (Fig. 2, Plate CLVI.). Some of these flattened bodies anastomose by these processes with those of other plates; others are quite free. The substance lying between the cells, the intercellular substance, is in the softest portions quite homogeneous or slightly granular, and has no marks of fibrillation. In the neighbourhood of the firmer tissue, lines of fibrillation occur, while at the same time these flattened bodies become smaller, though they are still flat.* The intercellular substance is distinguished by its chemical reaction, which distinguishes it from other albuminoid substances. It differs from albumen in not containing sulphur, from chondrin and gelatin in not being precipitated by boiling, tannin or the bichloride of mercury.

The corpuscles appear to consist of an oval, flattened, central body, about which there is an extremely delicate and pale envelope, that may or may not be connected with other similar bodies. These delicate bodies are smaller the nearer they are found to the firmer or fibrillated tissue, while as they diminish in size there appear under them certain areas of intercellular substance having the form of elongated and flattened bands, which, seen in profile, give to the whole the appearance of a spindle cell of which the flattened body is the nucleus (Fig. 3, *a*). That this is an illusion, however, may be judged from the fact that the flattened band will often be found to show the marks of fibrillation, and the flattened body may be seen to be simply superimposed *on* the band and not *in* it, for, by carefully brushing these tissues with a camel's-hair brush, after the prolonged use of Müller's fluid as above mentioned, and the subsequent immersion in a solution of common salt (10 per cent.), the bodies may often be brushed away (*b*).

* The intercellular substance in the figure is imperfectly represented.

Teasing of the tissue will often show isolated bands of more or less fibrillated tissue, and having no central body that can be seen even with the use of strong staining solutions; these evidences therefore seem to show pretty conclusively that such bands are not the bodies of the cell, as often stated, but rather portions of the intercellular substance in which fibrillation is commencing. About the flattened body will also be seen the remains of the envelope, either as a delicate film about it or in the form of irregular processes, projecting in various directions. According to this view of the case therefore the original flattened body or "nucleus" is at first surrounded by a delicate envelope, "the body of the cell;" the former undergoes comparatively little change, while the latter may almost entirely disappear. The fibrillation, however, appears first in the intercellular substance, the flattened corpuscle apparently never taking any part in it. As the tissue becomes more fibrillated and consequently firmer, the little plates diminish in size and are farther apart.

2. *Fibrous Tissue* (Fig. 4).—This substance, which is also known as fibrillated connective tissue, occurs either in parallel bundles or in networks. The latter variety may be shown exceedingly well in the umbilical cord of an infant at birth. If the same method is pursued as in the former case, excepting that a cut be made through the spongy portion of the cord, the following appearances will be noticed.

It will be seen that the tissue is composed of bright, shining, branching bundles (*d*), superimposed upon which are a number of oval flattened plates (*a*) at intervals; about them is a delicate envelope (*b*), which appears to be highly elastic, so that it will stretch or relax according as the networks are compressed or dilated. By teasing with needles, or immersion for a few days in a 10 per cent. watery solution of common salt, these corpuscles can often be separated from the bundles, and then they will be seen to form a connected system. When entirely isolated from one another, they often appear spindle-shaped. That this is not their character may be shown by *passing a current of fluid through the specimen*, which is done by the simple method of irrigation; that is, having affixed small strips of filter paper to the edges of the cover, and moistened one side with fluid, the excess will be absorbed by the other slip, causing a current by which the corpuscles may be made to roll over. We then learn that they are disks of an irregularly flattened form, having longer or shorter processes (*c c*)—variations in form which seem to depend in a great measure upon the tension to which they are exposed, and the position they occupy in the tissue. This explanation will serve to show why all measurements of such corpuscles are merely approximative, and have but little value. The nucleus may be regarded as an

exception, for it seems, in fresh specimens, when the substance has been swollen by immersion in water, to be oval and flattened in whatever position it is placed. The bundles upon which these bodies are placed are cylindrical in form, branched, and composed of separate filaments, which can be separated by Müller's fluid, or a 10 per cent. watery solution of common salt. Two other forms of corpuscles may also be noticed, the kind observed by Waldeyer (*loc. cit.*), and thought by him to be those that take up fat to make fat tissue, bodies four or five times the size of a lymphoid corpuscle, and rounded in form, containing a central body, and the ordinary lymphoid corpuscles seen at times in all tissues.

The form of fibrous tissue that occurs in parallel bundles is well shown in the mesentery of the frog, and in serous membranes generally. No great difficulty will be met with in preparing this tissue, for it is only necessary to remove it from the frog in the fresh state, acidulate it in a weak (1 per cent.) watery solution of acetic acid, and mount it in glycerine.

It will be seen that these so-called spindle cells are really flattened plates when viewed flatwise, and generally of an irregularly quadrilateral form, though the form varies somewhat in each instance (Fig. 5). [What relation these corpuscles bear to the interfascicular lymph-spaces described by Klein was not determined, as the silver method was not used. The bodies here described correspond very closely with those figured by this author, who regards them as standing in the radicles of the lymphatic system. 'Anatomy of the Lymphatic System,' ii. p. 7.]

3. *Adenoid Tissue* (Fig. 6).—Adenoid tissue is the name given to the delicate substance that forms the framework of the lymphatic glands. It consists of networks of fibres forming an intricate meshwork, that is filled with the rounded bodies commonly known as lymphoid cells. It is exceedingly difficult to analyze these tissues, owing to the fact that, with the exception of the lymphoid corpuscles, it is often hard to make out anything that conveys to the eye the idea of a cell body in the usual sense of the term. The best mode of procedure was found to be the following: Take a lymphatic gland that is in the early stage of inflammation, as an inguinal gland, for instance; harden it at first in Müller's fluid, and then in alcohol, and make sections through it. On viewing such a section with the microscope, it will be seen that it is formed of a delicate meshwork containing numbers of lymphoid corpuscles (*a*). By taking such a thin section and agitating it in a test-tube with water for a considerable length of time, and then placing it upon a glass slide and brushing it with a camel's-hair brush, most of the lymphoid cells will be removed, and the delicate network will be more thoroughly exposed. It will be seen that, at certain parts in this meshwork, there are flattened bodies (*b*) of

small size lying upon the larger parts of the meshes. It is held by Klein and other histologists that these are branching corpuscles, but it is by no means clear that this is always the case. In some instances this appearance is well seen in those portions of the glands that are regarded as the lymph passages, where the adenoid tissue forms the framework of the part. These fibres are extremely delicate, like fine silken cords, forming meshes which enclose vast numbers of lymphoid corpuscles, and appear to exhibit corpuscular bodies at the nodal points of the meshes. These delicate fibres, however, are often replaced by heavy cords (*c*), such as are seen in the drawing; and after continual inflammations the diameter of the cords may be found to be greater than that of the spaces. In these latter cases, it is often difficult to find any corpuscular elements that may not be separated from the fibres; and, indeed, large areas of these fibrous networks may, by diligent brushing with a camel's-hair brush, be swept clean of corpuscles. But neither this rough method, nor agitation in a test-tube, will always succeed in separating the corpuscles from the fibres, even after an immersion in common salt solution for many weeks. I do not therefore feel quite satisfied in thinking that adenoid tissue does not consist of branched corpuscles; but it is quite clear that the so-called networks of cells are at times replaced by networks made up of branching bundles of fibres, and in which the corpuscles play a minor part. Whether in such cases the bundles are made by the splitting up of the corpuscles, or, on the other hand, they are formed about the corpuscles, I do not feel prepared to decide. In my individual opinion, I must incline to the latter view as more in accordance with the appearances that are seen in the growth of reticular tissue, as I have had an opportunity of studying it in the umbilical cord. Where the fibrous networks have attained some thickness, there it seems that we find the ordinary flattened connective-tissue plates lying on the bundles, and surrounded by a delicate envelope in some cases.

It is not inconsistent with this theory that some, at least, of these lymphoid corpuscles may originate from the flattened corpuscles of the adenoid tissue, for it appears sometimes as if this production of the corpuscles could really be seen.—*Read before the Biological Section of the New York Academy of Sciences*, May 1, 1876.

(*To be continued.*)

PROGRESS OF MICROSCOPICAL SCIENCE.

Structure of Fossil Coal-plants.—Professor Williamson, F.R.S., continues to give the Royal Society valuable papers on this subject. The last presented was upon Ferns and Gymnospermous Stems and Seeds. The author described the stem of a new fern, in which the principal vascular axis formed a cylinder enclosing a medulla, as in some Lepidodendra. This vascular cylinder gives off secondary bundles, to petioles, and rootlets, and each vessel is filled with tylose. Two kinds of Fern-sporangia were described—one Polypodiaceous, with a straight, vertical annulus; the other, with the annulus horizontal and subterminal, exhibits a type seen in the recent Schizæaceæ and Gleicheniaceæ. But the chief subjects of the memoir are the stems and seeds of Gymnosperms. Of the former various modifications of the *Sternbergian Dadoxylons* are described, and shown to correspond very nearly to many recent conifers, though with distinctive features of their own, especially in the structure of their woody fibres, and in the leaf-bundles of some species being given off in pairs. The author still excludes the Sigillariæ from the Gymnospermous group. The most important novelties are the Gymnospermous seeds, exhibiting their internal organization, found in France by M. Grand-Eury, and by the author in this country. Of these he describes a number of new genera and species in addition to the Trigonocarpons previously described by Mr. Binney and Dr. Hooker. The most remarkable of these is one designated *Lagenostoma ovoides*, in which a large flask-shaped cavity, enclosed within a crenulated canopy, occupies the apical end of the seed, between the apex of the endosperm and the exostome. Brongniart believed, with reason, that such cavities have originated in the absorption of the apex of the nucleus, leaving the corresponding part of the nucular membrane to form the cavity or "lagenostome." In this lagenostome large pollen-grains are found in many cases. Brongniart designates it the "Cavité pollinique." Examples of several other seeds presenting generic and specific modifications of the same type, as well as several species of the well-known genus Cardiocarpum and of Trigonocarpum. In all these the primary nucleus seems to have been absorbed, being now only represented by the investing nucular membrane. Within this is an inner structureless bag, which, in some of the Cardiocarpa, is filled with parenchyma, and which appears to represent the *secondary* perispermic membrane, or what is really the endospermic membrane, or *primary* embryo sac of the Gymnosperms. The intimate structure of Trigonocarpum agrees with Dr. Hooker's description of it so far as the longitudinal sections are concerned, save that here, also, a "cavité pollinique" exists. Transverse sections show that the well-known sandstone casts of Trigonocarpum do not represent the external form of these fruits, but are *casts of the interior* of the hard endotesta. This latter was not trigonous externally, like the common specimens, but had twelve longitudinal ridges, three of which, corresponding with

those of the sandstone casts, were more prominent than the rest. The endotesta was invested by a delicate parenchymatous sarcotesta. All these seeds appear to have Cycadean rather than Coniferous affinities. One winged seed alone (Polypterospermum), from the uppermost coal-measures at Ardwick, resembles a true conifer. In conclusion, the author calls attention to the number of yet unknown stems and leaves of Phanerogams, which must have belonged to the numerous seeds now known to exist in the coal-measures of England, France, and North America.

Multiplication by Fission in Stentor Mülleri.—Mr. J. D. Cox has an interesting paper on this subject in a recent number of the 'American Naturalist.' He says:—I had the good fortune, one evening lately, to observe the whole process of the division of a large *Stentor Mülleri* into two complete individuals, by fission. The circumstances were favourable for pretty carefully noting the phenomena exhibited as the change went on, and there were some of them which I have not seen narrated, and which have a direct bearing upon the question of the organization of this group of infusoria.

The water was from the Maumee River at Toledo, Ohio, on Lake Erie, and contained a good variety of infusoria and of rotifers, which had propagated quite rapidly in the glass jar, among some aquatic plants carelessly thrown into it. The specimen of Stentor under consideration attracted my attention by its size, as it was about four hundredths (·04) of an inch in length, the stalk being stretched till it appeared about one-half longer than the proportions shown in the engraving of *Stentor Mülleri* in the 'Micrographic Dictionary.'

Whilst examining other forms in the compressor, I returned to this from time to time to enjoy its beauties, and soon noticed that the ciliary motion was extending from the disk, at the point of depression in its horseshoe shape, down along the body about one-quarter of its whole length, and this gradually became more marked. The portion of the body immediately under the disk swelled slightly, and the general form somewhat resembled the flower of the calla lily. The next change noticed was that at the bottom of the slit in the side the opening took a rounded form, so that the chain-like motion of the cilia looked (as a member of my family expressed it) as if a chain were running over a little pulley, and the cilia made a continuous fringe around the disk, down the body, and around the circular end of the slit. The body now began to show a protuberant swelling immediately under the small circular opening at the lower end of the ciliated slit, and in a few minutes this enlargement equalled in diameter the previous thickness of the body of the Stentor at this point, thus doubling its size at the point of greatest expansion. The protuberance was distinctly on one side of the body, and appeared as an excrescence, the ciliated line running out to its apex. The swelling continued to increase, involving gradually the whole circumference of the body of the animalcule, the upper side of the protuberance assumed a sharper angle to the longitudinal line of the body, becoming more disk-like, while the line of the cilia enlarged so as to show an approach to the general form of the original head of the

Stentor, the new oral opening gradually enlarging and deepening. The slit line between the two disks now disappeared, and at the end of another quarter of an hour the upper portion of the body was attached to the lower by a connection no thicker than the tail of the original had been, though in each the disk was about one-third smaller than the original disk, and the slope from it to the smaller part of the body below was much less abrupt than in the usual stretched form of the animal when its disk is expanded. The oral opening of the lower disk was now plainly seen to connect with the general internal canal by a circular orifice which varied in size, sometimes disappearing as if closed by a sphincter. Up to this period of development the Stentor had kept its place, attached by its tail to the upper glass of the Wenham compressor, its body stretched at great length, its cilia in rapid and vigorous motion, the whole animal waving slowly or partially rotating on its longitudinal axis. Now, however, it quickly retracted with a spring like the recoil of a bit of stretched india-rubber, in the manner common to it and the smaller Vorticellæ which have long pedicels. The two parts of the body, or more properly the twin bodies, enlarged in diameter while shortening in length, and it was apparent that the mass of each was about equal to the other, although the lower part had been more than twice as long as the upper when the whole had been stretched at full length. The form of the parts was now almost exactly alike in each, and resembled the common bell-shaped Vorticellæ, such as *Vorticella campanula*, &c. In the retraction the internal canal, which now became plainly visible, also enlarged in diameter when relieved from the stretch, and appeared slightly convoluted. It passed out from the lower body just below the margin of the disk, and entered the upper body at its caudal extremity, apparently having only an extremely thin membranous wall at the point of junction of the two bodies. These bodies now began a sort of swaying and gyratory motion, the lower one still fast to the glass by its tail, and the upper one swinging slowly around, the umbilicus between the two becoming smaller and smaller as if twisted up. Suddenly the connection parted, and the two Stentors swam separately away, both assuming the common form of the animalcule when free-swimming, and differing from the original individual only in being of smaller size. The complete transformation through all the stages I have noted occupied about two hours. I did not observe any internal difference of structure at the point where the swelling first began. No distinctly marked internal canal or sac could be seen when the body was stretched to its full length, but the manner in which it became unmistakably visible on the sudden retraction before the final separation of the parts looked strongly as if it had been there, but was drawn out to such tenuity as to be no longer apparent through the semi-translucent body.

Again, there was no doubt in regard to the fact that the ciliated line or slit extending from the disk down the body of the animalcule became apparent only after it had been some time under observation, and that the length and activity of the cilia along it increased rapidly

within a very few minutes, so as to become a striking and marked feature of its appearance. This raises the question whether the fringe of cilia down the body, as described, is a specific characteristic of the *Stentor Mülleri*, or is not rather a mark of the beginning of fission in all Stentors,—a question which an amateur naturalist may state, but will not presume to express an opinion upon.

In the instance above reported it is noteworthy that, except in the first appearance of the ciliated line down the body, there was nothing resembling a division by cutting or splitting. The body was of larger diameter than before, both above and below the new disk, when it first assumed the form of a protuberance with a ciliated circle on its anterior side; and the subsequent diminution of the diameter of the body and tail of the upper individual was gradual throughout its length, through the stages shown by the drawings.

The observations were made with a $\frac{1}{4}$-inch objective of low angle, but excellent definition and penetration, with the B eye-piece, and the situation of the Stentor in the compressor was very favourable for an unobstructed view of the phenomena at all stages.

Swedish Podurans.—The Poduridæ, or "spring-tails" of Sweden, have been monographed in an elaborate way by T. Tullberg. The memoir is accompanied by twelve plates, and enters quite fully into the anatomy of these little creatures of so much interest to microscopists. The work appears in the Transactions of the Royal Swedish Academy for 1871.

The Histology of certain of the Corallinaceæ.—At the meeting of the Linnean Society on June 15, Professor Duncan, F.R.S., delivered an oral epitome of a joint research by himself and Major-General Nelson, R.E., on some points in the histology of certain species of Corallinaceæ. Quekett, about 1851, gave a good account of the minute textural peculiarities of the hard structures of corallines generally, and in 1866 Rosanoff published a memoir on the Melobesiæ, therein bringing to light many details of the softer structures omitted by the former. Major-General Nelson and Professor Duncan now supplement the foregoing by further microscopic investigations on the living forms of Bermuda and Britain. On the shores of the former island the high and constant temperature conduces to a development and growth of the corallines not witnessed on our own seaboard, and the colours, moreover, are rich in proportion; for these and other reasons a more complete study of their development and physiology has been made. Starting from Quekett's and Rosanoff's labours, the recent researches show the presence of remarkable filamentous appendages to the dermal layer, which latter is composed of a loose cellular envelope, permitting the existence of large sub-dermal areas. The interior more aggregated cellular substance has certain radiating fibres running through, and which are modified at the joints. The growth of the cell-structure, semilunar bodies developed in the primordial utricle, the manner in which the deposition of carbonate of lime takes place, and other interesting facts, the authors elucidate and place on record.

Proportions of Red and White Blood-corpuscles in Health and Disease.—The instrument which we described some time ago for estimating the number of blood-corpuscles has been turned to practical account by its inventor, M. Hayem. The 'Lancet' (Aug. 5) says that M. Henri Bonne has described a series of observations on the proportion of white to red blood-corpuscles in different diseases, made under the direction of M. Brouardel. The examinations were made daily by the methods of MM. Malassez and Hayem. Among the facts recorded are the following. In a patient with cancer of the breast, before its removal by operation, the white corpuscles were 1 to 48 red; three days afterwards they were 1 to 28 and 1 to 23. When suppuration was established the proportion fell to 1 in 60, 1 in 90, and at last, when the pus ran freely, 1 in 400. In two cases of iliac abscess the leucocytes, before the abscess was opened, were 1 to 18 in one case, 1 to 38 in the other. Immediately after, the white corpuscles fell to 1 in 132 in one case, and to 1 in 130 in the other. In other abscesses the same result was obtained. It thus appears that the formation of pus in an abscess coincides with a considerable increase in the leucocytes in the blood, and that the increase disappears when the abscess is opened. Similar results have been found in other suppurative maladies. In small-pox on the fifth day the leucocytes were 1 in 450; on the sixth day 1 in 48; on the seventh day 1 in 150; on the ninth day 1 in 236. In a case of suppurating pneumonia the white corpuscles at death on the ninth day were 1 to 40 red. Other influences besides confined suppuration cause leucocytosis. An eruption of herpes in one patient raised the number in four days from 1 in 80 to 1 in 90. In typhoid fever the leucocytes are very numerous about the seventh day, but fall to from 1 to 70 to 1 to 500 by the seventh day. Their number does not coincide with variations of temperature.

On the Microscopic Observation of Minute Objects.—At the meeting of the Academy of Sciences of Philadelphia (May 9), Professor Frazer remarked that he desired to put on record a thought relating to Helmholtz's now famous establishment of the limit of vision through the microscope. As this limit was determined by half the length of a wave of light, and since the wave-lengths of the most refrangible rays of the light spectrum (i. e., the violet) are somewhere near the $\frac{1}{57000}$ part of an inch, the conclusion was reached that nothing more minute than the $\frac{1}{114000}$ part of an inch could be seen. But actinic waves or others of smaller length (of greater refrangibility too) in passing through a substance on which are lines or other markings less than $\frac{1}{114000}$ inch apart, may be altered to light waves, and become visible, provided that the substance through which they pass is capable of fluorescing—i. e., increasing their wave-length—and provided the distance apart of the marks to be seen is not less than one-half the wave-length of such actinic waves.

Death of Professor Ehrenberg.—Although this is not the place for a necrological notice, we feel that an exception may be made in the case of Herr Ehrenberg, an Honorary Fellow of our Society,

who has recently died (June 27) at Berlin, at a very advanced age. Christian Gottfried Ehrenberg, Professor in the Berlin University, and perpetual Secretary of the Berlin Royal Academy of Sciences, was born the 17th April, 1795, at Delitzsch, in the province of Saxe. According to the wish of his father he had made up his mind to become a clergyman, but, shortly after he had been enrolled as a student of theology at the University of Leipzic, he changed his purpose and devoted himself to the study of medicine. From the earliest period of his medical studies (says the 'Medical Examiner,' which has given a sketch of his life), he showed a strong predilection for microscopic investigation, and the first of his valuable discoveries was made shortly after he passed his "State Examination." He proved clearly in a paper, "De Mycetogenesi," that mould and low fungi arise from sperm and germ cells; and so in his first work he began that opposition to the theory of the "generatio æquivoca," the total defeat and abolition of which have secured him a well-earned immortality. This grand result was obtained, after some interesting scientific journeys with the General von Minutoli, and afterwards with Alexander von Humboldt, by his work, 'The Infusoria as Perfect Organisms.' In this work he proved again, first, that these beings also arise from sperm and germ cells, and then, by his celebrated feeding experiments with coloured materials, that they take nourishment. Without entering into particulars, it is at once apparent of what immense value the discovery of the presence of germs everywhere—in air, in water, in soil—must have been in connection with the theory of infectious diseases; and, indeed, it is not too much to say that all dogmas, questions, and theories on the subject of infection, putridity, bacteria, &c., take their origin from Ehrenberg's above-named book. Besides the above-mentioned, Ehrenberg was engaged in many very interesting works and investigations in different departments of natural history; for his life, until the severe illness of his later years, was a very industrious one. Science progresses rapidly, and soon many of them will be forgotten or have a mere historical interest; but Ehrenberg's name will always be connected with one of the most important scientific discoveries of the nineteenth century.

Termination of Nerves in the Skin of Mammals.—Dr. G. Thin, writing in the 'Medical Record' of May 15, states that M. Mojsisovres (whose paper is published in the 'Sitz-bericht der K. Acad. der Wissen.' band lxxi.) made his observations on the sensitive hairless skin which covers the central part of the snout of the swine. Nerve-fibrils from the papillæ, or direct from the cutis, enter the rete Malpighii. Some of them ascend in a winding manner towards the surface of the epidermis, becoming smaller and varicose as they approach the horny layer. Some send branches downwards, which anastomose with those which have entered at the base between the papillæ. The fibrils that go towards the horny layer bifurcate. They do not appear to anastomose. They pass between the cells, approach very close to the horny layer, and some of them seem to end in club-shaped swellings, which are similar to those described by Cohnheim in the corneal epithelium. After maceration in a 35 per

cent. solution of caustic potash, the cells disappeared, leaving the nerve-skeleton with its branches and end swellings intact. A corresponding arrangement was found in the epithelium of the hair-sheaths.

The Fœtal Placenta of the Pachyderms.—M. Dastre has given the results of his researches to the Paris Biological Society. He states that in the pig the villous folds which constitute the placenta are disposed in a radiating manner around the little bald or smooth centres. The structure of the stroma of the chorion is modified at these points. Moreover, the villosities do not extend over the whole surface of the chorion : they leave free in one part and another of the middle zone two ill-determined bands. Each half of the sac of the chorion is divided into three zones: a middle zone, vascular and villous; a zone vascular only but not villous, a continuation of the former; finally, a third zone (horns of the chorion), which is neither vascular nor villous. In the thickness of the stroma of the chorion of ruminants and pachydermata, there is found, in addition, a network of chalky aspect, formed principally by a deposit of phosphate of lime. The chorion plates of this deposit are in an intimate relation with the function of the fœtal placenta. But among the pachydermata they occupy the same situation as the villosities: they exist only in the middle zone.

Microscopic Anatomy at Berlin.—It would seem from the correspondence of one of our medical contemporaries the subject of microscopic anatomy is not getting on as well as in former years it was wont to do. It is alleged that at the beginning of the last summer term a new professorship of Microscopical Anatomy was founded for the sole purpose of giving this place to a certain person, under the pretext that the present Professor of Anatomy was so busy that he could not manage both general and microscopical anatomy. But there was some difficulty to overcome, which the influential friends of the *protégé* do not seem to have thought of. The present Professor of Anatomy, indeed, does not much care for microscopical teaching, and his assistants do not seem to be able to attract students to their own microscopical courses; for when the dreadful time of examination comes near, the students are accustomed to attend one or two courses of normal microscopical anatomy under the assistants of the celebrated Pathologist. The latter gentlemen, seeing a respectable source of income threatened by the mysterious new professorship, addressed themselves to their chief, who was joined in his efforts to avert this misfortune from their innocent heads by his colleague, the not less celebrated Professor of Physiology, who believes, of course, that a new post for teaching microscopical anatomy is quite superfluous, because, if once the magnificent new building of the Physiological Laboratory is finished (which it is shortly expected to be), there is space enough in his own rooms, and physiological assistants enough, too, to teach curious students microscopical anatomy.

The Cement-substance of so-called Endothelium.—In the January number of Virchow's 'Archiv,' p. 77, is an important paper by

Dr. J. Arnold, who has made further researches into the nature of the substance uniting the so-called endothelium cells (epithelioid cells). For this purpose he has employed a new apparatus by which extremely small quantities of colouring matter were injected for long periods into the living animal. The materials used were a solution of sulph-indigotate of soda, ferrocyanide of potassium, the part afterwards being washed with chloride of iron, so as to precipitate prussian blue, and thirdly, indian ink. Frogs and guinea-pigs were the animals experimented on; and the blood-vessels, lymph-sacs, peritoneal cavity, anterior chamber of the eye, and subcutaneous cellular tissue, were the parts into which the fluids were injected. In the case of the vessels, he obtained not only a network of coloured lines corresponding to the junction of their epithelioid cells, but also similar junction-lines in the perivascular sheath, as well as injection of this sheath, and the lymph-canalicular system of the neighbouring connective tissue, whence some of the fluids used passed into the serous cavities.

Ciliated Pus-cells.—In a communication made to the 'Central-blatt' (June 10) Professor E. Neumann, of Königsberg, states that if a catarrhal condition of the mouth and throat of a frog be established by the application of a few drops of a weak solution of osmic acid (one-quarter to one-half per cent.), the secretion of the mouth will be found to contain, in the course of from twenty-four to forty-eight hours, besides numerous slightly browned but otherwise unaltered epithelial ciliated cells, cup cells, and, besides amœboid cells presenting the usual characters of pus-cells, peculiar cells of an intermediate kind, which resemble the former in having cilia, and the latter in their contractility. The 'Lancet' (July 29) states that the cilia do not cover the whole surface, but form a kind of crown, or are compressed into a brush. They are imbedded directly in the protoplasm without the intervention of any basal seam. The cells often rotate actively in consequence of the play of the cilia, and then preserve an approximatively round form; but as soon as the cilia cease to play they begin to perform amœboid movements.

Dr. Bastian's further Results.—Besides a paper in the 'Comptes Rendus,' which has called out one from Professor Pasteur, who takes Dr. Tyndall's side, Dr. Bastian has communicated a long article to the Royal Society (June 15) on the subject of Heterogenesis. He states among his conclusions that the generally received belief that all Bacteria and their germs are killed by exposing them even for a minute or two to the temperature of 212° F. (100° C.) has of late been strongly reinforced by Professor Tyndall. The fact, therefore, of the fermentation of some specimens of boiled acid urine, with the appearance of swarms of Bacteria, under the influence of the high generating temperature of 122° F. (50° C.), is inexplicable except upon the supposition that fermentation has in these instances been initiated without the aid of living germs, and that the organisms first appearing in such fluids have been evolved therein. If the author's further position,* that Bacteria and their germs are killed in fluids whether

* 'Proceedings of the Royal Society,' Nos. 143 and 145. 1873.

acid or alkaline at a temperature of 158° F. (70° C.), is correct, then the occurrence of fermentation in the previously neutralized boiled urine would similarly disprove the exclusive germ-theory of fermentation and establish the occurrence of Archebiosis. Any difficulty which might have been felt by others in accepting the above interpretation of the results of these latter experiments—in face of the view held by M. Pasteur that some Bacteria germs are able in neutral fluids to survive an exposure to a heat of 212° F. (100° C.)—has been fairly met and nullified by the experiments (devised for the purpose), in which the urine was boiled in the acid state and subsequently fertilized by the addition of boiled liquor potassæ. If we look at these latter experiments from an independent point of view, it will be found that this fertilization of a previously barren fluid by boiled liquor potassæ must be explained by one or other of three hypotheses:

1*st Hypothesis. The boiled liquor potassæ may act as a fertilizing agent because it contains living germs.*—However improbable this hypothesis may seem on the face of it, it has been actually disproved by many of the experiments recorded in this memoir. These experiments show that boiled liquor potassæ will only act as a fertilizing agent when it is added in certain proportions. If it acted as a mere germ-containing medium, a single drop of it would suffice to infect many ounces, a gallon, or more of the sterilized fluid. This, however, is never the case.

2*nd Hypothesis. The fertilizing agent may act by reviving germs hitherto presumed to have been killed in the boiled acid urine.*—The acceptance of this hypothesis would involve a general recantation of the previously received conclusion that Bacteria and their germs are killed by boiling them in acid fluids. But such a recantation would be scarcely justifiable or acceptable unless based upon good independent evidence. The possibility, however, of accepting this second hypothesis is still further closed by the results of experiments in which a slight excess of liquor potassæ was added to the boiled urine. Such fluids invariably remained barren.

3*rd Hypothesis. The fertilizing agent acts by helping to initiate chemical changes of a fermentative character in a fluid devoid of living organisms or living germs.*—If the cause of the fermentation of the fluids in question does not exist in the form of living organisms or germs either in the fertilizing agent itself or in the medium fertilized, then it must be found in some chemical reactions set up between the boiled liquor potassæ and the boiled urine.

A New French Work on the Microscope has just been published by V. Masson, of Paris. It is entitled 'Le Microscope, son Emploi et ses Applications,' by Dr. J. Pelletan, with 278 figures and 4 plates. In reference to this book the 'Lancet' (Aug. 5) says:—"Microscopy has taken its place among the refined recreations of cultivated society. In the study, sometimes even in the drawing-room, the youth acquires a familiarity with the instrument which will yield many an hour of intellectual amusement in his rural or seaside *villeggiatura*. If preparing for the medical profession, he will at once feel the benefit of having thus early familiarized himself with an implement which has

wrought such a revolution in the sciences on which medical practice is based. In this stage of his microscopic experience he will find a good treatise on the mechanism of the instrument useful, and such a treatise has just been provided for him by Dr. Pelletan. In a portable octavo volume, Dr. Pelletan describes in detail the construction of the microscope, and the application of micrography to the study of the organic tissues, whether of the vegetable or lower animal kingdoms, and illustrates the exposition by numerous engravings selected with good judgment. The style of the volume, like that of most French scientific treatises, is clear and even attractive."

Do Plants Digest Animals?—Mr. Darwin's inquiries on this point are well known to our readers. 'Nature,' however, gives us an account of an opposing idea. It states that an interesting addition to the literature of insectivorous plants is furnished by a reprint, by Casimir De Candolle, from the 'Archives des Sciences Physiques et Naturelles,' "Sur la Structure et les Mouvements de Feuilles de *Dionœa muscipula.*" With regard to the power of digestion, M. De Candolle comes to a conclusion opposed to that of Darwin, that the absorption of animal substances is not directly utilized by the leaves, and is not necessary to the development of the plant. He considers their anatomical structure favourable to the hypothesis that the movement of the two valves of the leaf results from variations of turgidity of the parenchyma of their upper surface.

Contractile Vacuoles in the Vegetable Kingdom.—M. Maupas has published a recent paper on this subject. The contractile vacuole has been regarded as a characteristic of animality. But various recent facts are against this. M. Maupas describes contractile vacuoles he has found in macrospores of the algæ, *Microspora floccosa*, Thuret, and *Ulothrix variabilis*, Kützing (both in Algeria).

The Anatomy of the Ear is receiving considerable attention at this time. Two essayists are especially worthy of attention, Dr. Urban Pritchard, of King's College, and Dr. Victor Urbantschitch—a curious resemblance between the two names. And first of Dr. Urbantschitch. His paper is abstracted in a recent number of the 'Medical Record,' and it is stated as the result of the examination of the ossicles of fifty ears that the following may be taken as the average size of the different bones:

Hammer.—The length of the hammer was on an average 8·5 millimeters. The short process averaged 1·6 millimeter in length. The long process was in an individual thirty years of age 2·5 millimeters, and in one of twenty years 5·8 millimeters long. The manubrium, measured from the point of the short process to the lower end, ranged from 4·2 to 5·6 millimeters. The distance of the lower end of the manubrium from the periphery of the membrana tympani averaged in thirty-eight cases 3·5 millimeters from the inferior edge, 3·4 millimeters from the anterior border, and 4·6 millimeters from the posterior border.

Anvil.—The general distance of the upper end of the ambos-joint surface from the free end of the processus brevis was 5·3 millimeters,

and that of the lower edge of the same joint from the inco-stapedial joint 4·6 millimeters.

Stirrup.—The length averaged 3·7 millimeters; the breadth, measured from the middle of both limbs, 2·3 millimeters; the distance of the upper arch of both limbs to the base of the anterior limb 2·2 millimeters, to that of the posterior limb the same. The breadth of the anterior limb averaged 0·6 millimeter, of the posterior 0·8 millimeter. The length of the base was 3 millimeters, the breadth 1·5 millimeter.

The *tensor tympani* is inserted into the manubrium generally to the extent of from ·7 to 1·0 millimeter in breadth. The manner of its insertion was found to be very different. In nineteen cases it was inserted on the anterior surface, in twenty cases on the inner edge and anterior surface, in seven cases on the inner edge alone, in two cases on the inner edge and posterior surface, in two cases on the posterior surface alone, and in ten cases the tendon passed round the inner border, and was fastened on both the anterior and posterior surfaces.

The *stapedius* had an average length of tendon, measured from the summit of the pyramid to its insertion into the stapes, of 1·2 millimeter.

In reference to Dr. Pritchard's paper, which is of considerable length, and may be read in the 'Proceedings of the Royal Society' (No. 168), we shall merely give the author's observations on the development of the organ of Corti, and his mode of preparation. He states that the organ of Corti is developed from certain of the epithelial cells lining the ductus cochleæ, which at first consist of a single layer of cuboid cells; later on, those cells which line the floor and sides of the canal elongate into the columnar form, while those lining the inner part of the floor become longer still, and their nuclei multiply two, three, and fourfold. On the outer side of these are five cells, from which the chief part of the organ is developed; these he terms the five primary cells. The limbus is next developed and the sulcus formed, the latter being completely filled up by the tall columnar cells with proliferated nuclei; these, however, dwindle down again as age advances. The contents of the first or innermost of the five primary cells are divided into two transversely, the upper division forming the inner hair-cell, the lower a nuclear cell. The same change takes place in the third, fourth and fifth primary cells, their upper division forming the outer hair-cells, and the lower the cells of Deiters. The rods are developed from the second primary cell, which does not divide transversely, but widens at its base so as to form a triangular cell, the inner and outer sides of which thicken and form the rudimentary inner and outer rods. As this widening increases, the protoplasm disappears from the centre, forming the triangular tunnel, and the nucleus divides into two, one for each of the lower angles of the tunnel. The rods enlarge at their upper and lower extremities, but do not attain their perfect form until after birth; at this period the angle of the inner-rod head has not been developed, besides which the shafts are much thicker and more clumsy in form than in adult life. The membrana reticularis is

developed from the free border of the five primary cells and a few of the other adjacent tall ones, but the exact manner in which the reticulation is formed has not been made out. The trabeculæ are, like the rods, developed from the side of primary cells; and although only three (on the outer side of the rods) persist in adult life, yet at birth numerous fine trabeculæ are found to the inner side of the rods. He regards the membrana tectoria as a secretion from part of the general epithelial layer; it first appears as a thick but even layer, and as age advances that portion which covers the organ increases considerably in thickness. From the foregoing observations it will be seen that the rods, membrana reticularis, and trabeculæ are developed from the walls of the original epithelial cells, whereas the hair- and various other cells are formed from their contents; and, lastly, that the membrana tectoria is a secretion from the same original epithelium.

The method of preparation usually employed was as follows:

The cochleæ were always quite fresh.

They were hardened in a $\frac{1}{3}$ per cent. solution of chromic acid in ordinary methylated spirit; ten days required.

Decalcified in 1 per cent. solution of nitric acid in water.

Transferred directly to gum-water, soaked a few hours, and then placed in a paper bag surrounded by spirit.

Imbedded in Stirling's machine and cut.

Gum gradually dissolved away in proof spirit.

Mounted, stained or otherwise, in glycerine or Canada balsam.

He has examined the cochleæ of the following mammals: man, monkey, sheep, dog, cat, rat, guinea-pig, rabbit, porpoise, kangaroo. With the exception of the peculiarities in man and monkeys referred to, he has found a striking similarity in the organ of Corti of all these animals.

Unfortunately all his efforts to procure the cochleæ of a monotreme have as yet proved unsuccessful, a circumstance much to be regretted, as he fully anticipates that it presents some appearances which link the very dissimilar cochleæ of mammals and birds.

Microscopy at Philadelphia.—We have not yet received any report of the Exhibition; but we believe Mr. Crouch and certain other makers are represented. In regard to microscopic specimens we have no doubt that this department has been well arranged, particularly if the authorities have placed the matter in the hands of Colonel Dr. Woodward. We learn that there are some four hundred frames which contain fungi of all kinds, many of them microscopic.

The Dissection of Insects for examination of their Microscopic Anatomy.—Dr. Fripp, in an admirable paper on the subject of insect anatomy, gives the following account of the mode of going to work, which is of considerable interest to the practical observer.* He says: Much of our knowledge of internal structure has been gained by observation, under the microscope, of insects possessing a transparent integument. The larval forms are most suited for such examinations, and are best examined whilst living, when circulation or muscular

* 'Proceedings of the Bristol Naturalists' Society,' vol. i., part 3.

action is to be studied. If the parts to be studied do not require hardening, it is best to dissect them out immediately after death, glycerine being used to keep them transparent, for when preserved in glycerine the structures can be left to any convenient time for examination. When spirit is used, hardening occurs in a few days, after which the tissues shrink and become granulated and knotted together so as to break under dissecting needles; besides which they gradually get stained and opaque, so that they are not any longer well seen under the microscope by transmitted light. Water should not be used when dissecting, but the object must be floated in glycerine, and all fatty tissues removed as soon as possible.

The magnifying power under which dissection is carried on necessarily varies with the minuteness of the object — the lowest power under which the parts can be distinguished should always be chosen, because they can be kept better in sight and a firmer control over the movement of the dissecting needles exercised. With high power, the object escapes readily out of the field, and the needles are not easily brought to bear upon it. The power should be either a single lens, or a combination of lenses which magnifies without inverting the image.

Most insect preparations can be examined and made under a low power (5 to 25), but it is well to examine them under higher power during their preparation (50 to 75). When histologic elements are studied, still higher powers are needed. If, for instance, insect muscle is the object to be examined, the striation may be readily seen with quite low powers, but the arrangement of sarcous elements can be demonstrated only when powers varying from 400 to 800 are used. And by far the most beautiful objects are obtained when polarized light is used and advantage taken of the different refracting power of the disks and intervening substance. The ordinary striation of muscle fibre may be seen most perfectly in the muscles of the thorax, which naturally split up into long fibres, offering excellent specimens for study (e. g., in the common house-fly). For minute analysis of the sarcous mass, the muscles of mites (Trombidium) have been recommended, as the striation of such muscles is remarkably coarse and distinct.

Muscle must be taken from an insect immediately it is killed. It may sometimes be advantageously treated with alcohol, or osmic acid (weak solution) and prepared in glycerine. When studying the phenomena of contraction, which will be seen in various phases along the length of the fibre, the muscle should be examined either in living insects, or in recently removed parts immersed in blood serum or some albuminous fluid (white of egg, e. g.), or in glycerine, but never in water. In insect muscle preserved in spirit, especially if the insect has been dropped, while living, into the spirit, the varying state of contraction of different elements of the same fibre may be seen just as fixed at the time of death.

It frequently happens that the anatomist has not the opportunity of dealing with insects in the living or fresh condition. In such cases the specimen must be preserved in weak alcohol.

In the dissection of insects, different methods of treatment and manipulation must be adopted, according as it is desired to learn the structure of any particular organ, or to prepare and mount specimens, and economize so as to get the greatest number of preparations from a single insect. In learning the anatomy of any insect, not previously studied, a few specimens must be sacrificed by cutting and picking to pieces. But material may be saved by following some methodical plan. *External* parts can of course be studied as they present themselves, but it is worth while to preserve the dermo-skeleton, either whole, or in parts (sections), and this is best accomplished by boiling in solution of potash, then washing in cold water and removing with scalpel or brush any remaining soft parts (ligaments, membranes, tracheæ, &c.). To mount them, a suitable fluid, spirit, turpentine, balsam, glycerine, &c., must be used, and a convenient size and shape of cell chosen. The parts composing the mouth require special attention, and should be separately mounted. The whole head may be divided in various directions, yielding longitudinal, transverse, and horizontal sections, each displaying some particular aspect. For example, a longitudinal section shows external and internal lateral views of the cranium with eye, antennal first joint (the antenna being cut off), mandible, maxilla, and palpi. Cross sections yield anterior and posterior views of the internal processes, separating the cranial and facial halves. Horizontal sections show external and internal aspects of the vertex of the cranium with orbital and antennal sockets, labrum, &c., or of base of cranium with maxilla and palpi, gula, labrum, lingula, paraglossæ, &c. The study of such sections of the dermo-skeleton, from which the soft parts are removed by boiling in solution of potash, is a necessary preliminary to the study of the very intricate anatomy of the soft parts contained within them—e. g., tongue, pharynx, œsophagus with its salivary glands and muscles, supra and infra-œsophageal ganglia and nerves, together with a mass of intracranial muscles arising from the inner surface of the cranium and internal processes, and attached to œsophagus, palate, and mandibles.

The student will be greatly assisted, and valuable time saved, by purchasing all good insect preparations which he can obtain commercially. Such preparations will probably be far better made and mounted than those which he makes for himself. But no cabinet of preparations will teach that knowledge of insect anatomy which is to be gained only from actual dissection. The relation of the soft parts to the dermo-skeleton, and their own relative position to each other, can only be learnt in their entirety by those who dissect them in the fresh state and examine them *in situ.* Besides which, preparations of the most important and interesting parts are not usually made for sale, as they are difficult to dissect, and demand much time in preparing. But whilst dissecting for himself, the student will find every tissue and fragment of tissue well worthy of study from histologic points of view, as well as on account of their anatomical relations. For instance, it is always well to examine fragments of muscle fibre scattered in the field, for the chance of securing good

examples of nerve insertion under the sheath of the muscle, and distribution of its terminal filaments in the muscle substance. Fragments of gland structure sometimes offer unexpected yet beautiful specimens for preparation. The same may happen with varieties of gland in mucous membrane of intestines and other organs (testis, urinary tubes, hepatic tubes, &c.). Beautiful varieties of fatty tissue and dermoid tissues or exquisitely striated muscle (as in the coats of the dorsal vessel) or curious forms of tracheal ramification and wonderful networks distributed over or penetrating through other tissues may be secured as chance prizes, if looked for amongst the débris after the principal organs have been secured.

The dissection and removal of the soft organs and finer structure is however attended with considerable difficulty when enclosed in a casing of such tough and resistant chitin integument as forms the dermo-skeleton of Coleoptera, Orthoptera, Hymenoptera and other classes. Of course this difficulty is greatest at the natural cinctures which mark the chief divisions of the insect into head, thorax and abdomen, and wherever internal processes connect the anterior and posterior surfaces, as happens for instance in the case of the cricket, both in the head and the thorax. The abdominal organs are easily removed from almost every kind of insect, but their continuity with the parts contained in the thorax can be preserved only by skilful manipulation. As the alimentary canal, and nerve chord, extend from head to tail, the integument must be slit up from end to end on the ventral side, but not directly in the median line, because it is best to remove the ganglionic chord with the œsophagus and intestine. In the instance of the cricket, however, the intracranial portion of the œsophagus, with its closely adherent infra and supra-œsophageal ganglia, cannot be detached without first cutting off the head and very careful tearing out, as these organs rest upon a saddle-shaped osseous plate in the very centre of the cranial cavity. The ganglionic chord within the thorax is also enclosed between forks of internal osseous plates, but by following it up from the abdomen where it lies free, it can with a little care be got out entire. It is best to keep the whole insect floating in glycerine, the body being secured in any convenient way in a fluid-holding cell of suitable form.

To exhibit continuous systems of organs, and show their relative position to the several divisions of the external integument, longitudinal and transverse sections are useful. But these cannot be well made without a preparatory hardening process. If complete sections of the whole body are desired, the hardening process should be supplemented by soaking the parts in some material which will preserve them in unchanged position under the action of the knife. The following plan, which has been found most successful in making sections of diseased structures, and is employed by Professor Ranvier, appears to me the most suitable for the purpose.

Place the parts (or the whole insect, taking care to make such punctures or slits in the integuments as will allow the fluids used to penetrate thoroughly) in alcohol for twenty-four hours, a time sufficient to fix, without contracting the tissues. Then, in solution

of picric acid for a few days, by which the spirit is expelled and the parts are again slightly hardened. After a few days, wash in pure water and plunge the preparation into a weak solution of gum arabic, which completely penetrates the tissues in a few days. Then remove and place in alcohol, which takes up the water, and the gum solidifies and yields a mass which resists uniformly the cutting blade—microtome, or razor—according to convenience. When the section is made, the gum dissolves out after soaking a little while in water, and the preparation can be floated in the fluid selected for preserving it. Cells are of course needed for preparations of larger parts and organs.

Gymnospermous Seeds of the Coal-measures.—In Professor Williamson's recent memoir before the Royal Society (May 18), the author directs attention among other matters to the curious seeds discovered in America, and published in Professor Newberry's 'Geological Survey of Ohio.' These, however, merely display external forms. Still more remarkable is the collection of such seeds found by M. Grand-Eury at St. Etienne in France. These exhibit their internal structure in a wonderful manner, as is shown by M. Brongniart's brief memoir published in the 'Annales des Sciences Naturelles.' M. Brongniart called attention, in that memoir, to a remarkable organization of the micropylar extremity of many of these seeds, where a peculiar cavity existed, between the micropyle and the apex of the nucleus, into which the pollen-grains obtained entrance through the micropyle, and were thus brought into contact with the nucleus. In a more recent memoir on the fertilization of the ovules of some species of recent Cycads (Ceratozamiæ), M. Brongniart showed that a mammillar prolongation of the apex of the nucleus projected into the micropyle, which it filled; but that during fertilization the cells of this prolongation became disorganized, and a cavity was produced into which the pollen-grains found their way, the apex of the nucleus below this cavity becoming covered over by true perispermic membrane. These structural peculiarities so far accord with what he observed in M. Grand-Eury's seeds, as to lead him to surmise that the latter had Cycadean rather than Coniferous affinities. The author has found a number of remarkable seeds of a similar type to those from St. Etienne in the Oldham nodules, and he has been indebted to friends for a few others. The first of these is a very small, nearly spherical seed, which the author names *Lagenostoma ovoides*, about ·16 of an inch in length and ·1 in breadth. It has a solid testa, within which can be recognized two distinct membranes —an inner or "perispermic" one, which has enclosed the endosperm, and an outer or "nucular" one, which has been in close contact with the perispermic one throughout the greater part of the seed, but which splits up at its apex into two portions, the inner one of which forms a remarkable flask-shaped cavity, which the author designates the lagenostome. Its base has rested upon the apex of the perisperm, and its upper extremity has been continuous with the micropyle. Within this lagenostome is a little delicate parenchyma, which has shrunk up towards the centre of the cavity, leaving a surround ing space in which, in some examples, the author has found the objects

regarded by M. Brongniart as pollen-grains—an opinion in which the author concurs. External to the lagenostome the second or outer division of the nucular membrane forms a remarkable "canopy," which hangs down from the micropyle, enclosing the lagenostome within ten sharply defined and regular crescentic folds, the concavities of which are directed outwards. The walls of this lagenostome and of the "canopy" correspond with the nucular membrane in consisting of flattened prosenchymatous cells. The perispermic membrane, on the other hand, looks structureless, save that it appears to have had imbedded in it an innumerable multitude of minute crystals, like those observed by Dr. Hooker on the spicular cells of *Welwitschia.* A second species the author designates *Lagenostoma physoides.* In this the apex of the endospermic sac contracts into a mammilliform prolongation, overlapped by the base of the lagenostome, which overhangs it as a bladder half full of water might be made to overhang the neck of a soda-water bottle upon which it rested. This species has other distinctive structural peculiarities. For a second genus of new seeds the author proposes the name of *Conostoma. C. oblonga*, from Oldham, is about ·18 of an inch in length. Here, again, we have an endosperm enclosed in a perispermic membrane, and this in turn is encased within a nucular one, the whole being invested by a dense testa. The lagenostome is again formed out of divisions of the apical part of the nucular membrane; but it assumes a funnel-shape at its base, whilst its upper extremity is continuous with the micropyle. A second species, named *C. ovalis*, is from the Burntisland deposit, and is more ovate than *C. oblonga.* In it the lagenostome assumes a remarkably funnel-shaped contour. The same deposit has furnished a third species, *C. intermedia.* To another remarkable seed from Oldham the author gives the name of *Malacotesta oblonga*, of which the maximum length, exclusive of its funiculus, has been about ·25. Its exotesta has been soft and parenchymatous, with a prosenchymatous inner (nucular ?) membrane. The micropyle has been remarkably wide with incurved margins at the exostome, and enclosing a mass of delicate parenchyma through which a canal passed. The author has obtained a fine series both of longitudinal and transverse sections of *Trigonocarpum olivæforme*, the seed long ago made the subject of a valuable memoir by Dr. Hooker and Mr. Binney. So far as the longitudinal sections are concerned, the results obtained correspond closely with those already arrived at by these two authors, except that a modified form of lagenostome is shown to have existed at the apex of the nucleus. The transverse sections show that the two layers of the testa, an outer soft parenchymatous exotesta and an inner sclerotesta, present some striking features. The exterior of the latter has exhibited three principal, acute, prominent, longitudinal ridges, between each two of which are three intermediate ones, the centre of these three being rounded, and the two flanking ones acute. The internal cavity of the endotesta is prolonged like a narrow fissure only into each of the three principal ridges. The ordinary sandstone specimens of *Trigonocarpum olivæforme* commonly seen in cabinets do *not* represent, as has hitherto been supposed, the

exterior of these seeds, but are casts of the *interior* of the sclerenchymatous endotesta, the three thin, longitudinal, wing-like appendages being merely casts of the three slit-like extensions of that interior just referred to. These slits extend upwards into the prolonged micropyle, the interior of which displays a triangular section, each of the sides of which is convex, the convexity projecting inwards. The nomenclature of this type of seed is in great confusion, owing to specific differences being based on mere differences of size, many of which are probably nothing more than varieties due to age and development. Casts of seeds with six longitudinal wings are described, corresponding with Brongniart's genus *Hexapterospermum.* They are more oblong than *Trigonocarpum olivæforme*, but apparently identical with the *T. Nöggerathi* of the 'Fossil Flora.' The author doubts the wisdom of Brongniart's establishment of a separate genus for these seeds. Several species of the important genus *Cardiocarpum* have been obtained displaying the internal organization of these remarkable seeds. They all agree in possessing a central endosperm which is remarkable for the very large size of its conspicuous parenchymatous cells. This is invested by a perispermic membrane, the whole being enclosed within a testa composed of two very distinct and separate layers. A thin inner one, which may be identical with the nucular membrane of other seeds, is entirely composed of delicate prosenchymatous cells, and is prolonged into an elongated micropyle, into which the endosperm is not prolonged. Externally to this is an exotesta composed of a denser parenchyma. In some species this latter tissue is uniform throughout, in others it is separable into a dense endotesta and a more lax parenchymatous exotesta. The first species described is apparently identical with the *C. anomalum* of Carruthers, and has a trigonous endosperm invested by the two layers of testa (?), both of which are prolonged into a slender tapering beak, half the entire length of the seed, and which contains the elongated micropyle. Another species, designated *C. compressum*, has its apparent testa composed (as just described) of two continuous layers. In it the micropyle is comparatively short, and its apical extremity is patulous or trumpet-shaped. To a third very beautiful little cordato-lanceolate species with a peduncle or funiculus equal in length to the seed, the author gives the name of *Cardiocarpum Butterworthii*, after its discoverer. These seeds exhibit no specialized organ corresponding to the lagenostome of *Lagenostoma* and other seeds described. The pollen has passed down the long narrow micropyle into the triangular space at its inner extremity, where it came into direct contact with the endospermic membrane. It thus appears that the seeds known by the name of *Cardiocarpum* have a very simple organization, approximating somewhat closely to that of the ovules of *Juniperus*, *Callitris*, and *Welwitschia.* Some small seeds, which appear to be identical with the *Cardiocarpum tenellum* of Dawson, found in great numbers on slabs of shale by Mr. John Smith, of Kilwinning, in Ayrshire, are described. They were found in the upper coal-measures near Stonehouse in Lanarkshire. The last form noticed is a very curious winged seed from the uppermost coal-measures of

Ardwick, at Manchester, and which appears to have been a double seed, resembling in general form the samara of an ash. It belongs to Brongniart's genus *Polypterospermum.*

M. Pasteur's Reply to Dr. Bastian on the Heterogeny Controversy.—The 'Lancet' of August 5 has the following interesting note on the above subject:—A communication recently made by Dr. Bastian to the Paris Academy of Sciences, upon the influence of physico-chemical forces upon the phenomena of fermentation, has met with a prompt reply from M. Pasteur,* in a note on the changes undergone by urine, in which he points out what he considers to be errors in Dr. Bastian's experiments, which he has himself repeated. In his opening remarks, he contrasts the supporters of spontaneous generation with the theorisers in physics and mathematics who believe in perpetual motion or in the quadrature of the circle; and he thinks that the only reason why heterogenists are listened to at all is because of the impossibility of proving *a priori* the origin of life except from life—in other words, because biology is not yet an "exact" science. Turning to Dr. Bastian's paper, he singles out for especial comment those experiments in which bacteria appear in urine mixed with oxygen and solution of potash, and kept at a temperature of 122° F. (50° C.), the urine having been previously boiled, and all precautions taken to withdraw it from any source of contamination by atmospheric germs. M. Pasteur admits that Dr. Bastian's experiments are very exactly carried out, but he contends that a temperature of 50° C. is not sufficient to kill all the germs of minute organisms which may be introduced into the urine by means of the solution of potash. In his memoir, published in 1862, and entitled 'Sur les Corpuscles organisés qui existent dans l'Atmosphere: examen de la doctrine des Générations Spontanées,' he showed that acid liquids which are rendered sterile after a few minutes' exposure to a temperature of 100° C. remain fertile when made slightly alkaline; and he considers the precaution adopted by Dr. Bastian, of further heating the fluid to 50°, to be superfluous. He considers it fully proved that the germs of certain organisms which do not resist a temperature of 100° in acid solutions, are capable of such resistance in neutral or slightly alkaline fluids. In the latter case, in order to effectually destroy all germs, the solutions must be heated above boiling point—e. g., to 110° C. He therefore recommends Dr. Bastian to repeat his experiments with the additional precaution of adding solid caustic potash previously heated to redness or to a temperature of 110°, instead of employing the alkali in aqueous solution; or simply to heat the urine and liquor potassæ to 110° instead of 100°. M. Pasteur has found that perfectly normal urine rendered alkaline by a piece of solid potash undergoes no change when due precautions are taken to get rid of the contact of the atmospheric germs; and, in conclusion, he expressed the hope that Dr. Bastian would abandon his faith in spontaneous generation and in the proofs which he thinks he has given of it. At the same meeting M. Pasteur related observations showing that the

* 'Comptes Rendus,' July 17.

urine of a healthy man is free from germs, but in the majority of cases it meets with different kinds of germs at the orifice of the urethra, or in the air in the neighbourhood, during its emission. He also described the very simple apparatus which he had employed in repeating Dr. Bastian's experiments, in which he was aided by MM. Joubert and Chamberland.

An American Infusorial Stratum.—In a late number of the 'Proceedings of the Boston Society of Natural History,' Mr. Charles Stodder states that Mr. R. B. Tolles examined the stratum as it is exposed in a ravine on the west side of Shockoe Hill, near Richmond, and obtained specimens at the depths, 5, 7½, 10, 11, and 14 feet below the top of the bank, and also from the north side 40 feet below the top, from a bed which was apparently a continuation of the 14-feet bed, the hill being higher on the north side. The lower layer contains 50 to 80 per cent. of organic forms, the uppermost about 20 per cent. The species below this top layer vary but little; while in that they are partly different in species, and the frustules are less broken. The species of diatoms peculiar to it are: *Coscinodiscus perforatus, Aulacodiscus crux, Eupodiscus Rogersii,* and *Mastagonia actinoptychus.* Mr. Stodder gives a list of the species afforded by the several beds.

Dr. Tyndall and Dr. Bastian before the French Academy.—The 'Comptes Rendus' of July 31 contains the following extract, which is made from two letters sent by Dr. Tyndall to M. Dumas:—"M. Tyndall writes to M. Dumas on the 26th of July from Brigne, in the canton of Valais, that he has been surprised to learn from the 'Compte Rendu' of the 10th of July that Dr. Bastian points him out as bearing evidence to the exactitude of his experiments. He says that he finds, on the contrary, that at a temperature of 50° Centigrade, furnished by the Alpine sun, nothing supports Dr. Bastian's ideas. All that Dr. Bastian alleges in favour of spontaneous generation fails to manifest itself. In a second letter, dated July 29, M. Tyndall, after having read M. Pasteur's reply to Dr. Bastian, gives his entire adherence to our colleague [M. Pasteur], and calls on all enlightened persons to banish from science this doctrine of spontaneous generation, which has nothing whatever to support it."

The Characters of the Blood in Anœmia.—This has been carefully gone into by M. G. Hayem—whose blood-measurer, our readers will remember, we exhibited at the soirée of the Royal Microscopical Society—who draws the following conclusions from his paper before the French Academy of July 17. We may mention that he arrives at a result quite different from that of Dr. Wharton Jones—viz., that there is no relationship whatever between the red and white corpuscles of the blood. The conclusions are:—(1) The red globules are very alterable elements; (2) It results from their alterations, in chronic anæmia, that their feebleness of colour or of their power of colouring the blood and the defect of concordance between this colouring power and the number of coloured elements, are the only two essential characters of anæmia; (3) That if in the anæmia the total mass of

blood remains the same as in the normal state, which appears to be the case in the majority of instances, the determination of the colouring power alone gives an exact measure of the degree of anæmia; (4) It is useful to distinguish in pathologic physiology the modifications which are related to the generation of globules, from those which belong to the evolution of these elements.

He concludes his paper by saying that in anæmias of median intensity the formation of red globules, so far from being relaxed, is often more active than in the normal state. But they are attacked in the course of their evolution, which thus becomes incomplete. The case must be severe indeed where one sees a relaxation in the formation of red globules.

NOTES AND MEMORANDA.

A Microscope Stage and Lamp.—Mr. J. E. Smith, of Ashtabula, Ohio, U.S.A., writes to the 'Cincinnati Medical News' as follows:—Some nine years since, I purchased one of Zentmayer's "Army Hospital Stands." It has been in constant use to the present time. A few weeks ago I begged Mr. Zentmayer to make me a new revolving stage, one thin enough to admit oblique pencils up to 80° from axis. Mr. Zentmayer kindly responded to my request, and I now enjoy a very complete stand, serviceable for all work. To the many who own Army Hospital Stands, I would say, do not abandon them, but "go and do likewise;" and for the benefit of such I append a brief description of the new stage. It is $4\frac{1}{8}$ inches in diameter, and a little less than $\frac{1}{5}$ of an inch in thickness at the periphery, *decreasing* towards the centre to about $\frac{1}{32}$ of an inch at the well hole: the reasons for this are obvious. The stage consists of two plates, the lower one being securely attached to the "limb" of the stand, and is furnished with adjusting screws for centering; over this the upper plate is fitted concentrically, and revolves in the optical axis of the instrument; this plate in turn supports the object carrier which traverses the revolving plate with easy and smooth friction, regulated by a binding screw; the carrier is also furnished with a removable stop, for use with the Maltwood finder. Another plate, about 3 inches in diameter, fitted with tube for carrying sub-stage appliances, connects by a bayonet catch to the under surface of stage, and has a sliding movement for centering purposes; this plate can be placed in position or removed therefrom instantly. Notwithstanding the extreme thinness of this stage, I find it to be sufficiently firm. Those, however, who expect the solidity found in English stages, $1\frac{3}{4}$ inch thick, will be disappointed.

I have lately modified my German student's lamp, and get illumination superior to the round Argand burner. The change is easily made with any model of student's lamp, having removable burner, and the cost will not exceed fifty or seventy-five cents. A friend called

my attention three or four weeks ago to a novel arrangement of a bull's-eye condensing lens, in connection with lamp and mirror. At first sight I was pleased with this method of illumination, and have given the idea considerable study, resulting in a slight change of the arrangement suggested for oblique light work, especially the resolution of difficult diatoms, such as Nos. 18, 19, and 20, of Möller's (balsam) Probe-Platte. The method will be found very superior, easily manipulated, and inexpensive.

Cement for Glycerine Mounting.—Mr. Kitton, whose authority on this subject is admitted, gives the following piece of advice in a recent number of 'Science Gossip':—White lead in powder, red ditto in ditto, litharge in ditto,—equal parts of each. These are ground together with a little turpentine until thoroughly incorporated, then mix with gold size. The mixture should be sufficiently thin to work with the brush; it is perhaps scarcely necessary to say that the edge of cover and slide should be free from moisture before applying the cement, and the first coat allowed to dry before putting on a second. The last can be applied somewhat thickly, or, as the japanners say, floated on. No more of the cement should be made than is required for present use, as it soon sets and becomes unworkable. To save the trouble of grinding, a stock of the mixture can be kept ready ground in a bottle.

M. Cornil's Experiments on Staining Materials.—In a memoir devoted to the subject of amyloid degeneration of the kidney, liver, and spleen, which appears in a recent part of the 'Archives de Physiologie,' M. Cornil gives the results of his experiments with several new colouring matters. Two of these were methyl-aniline violets discovered by M. Lauth; the third was a violet discovered by Dr. Hofmann, of Berlin. The preparations can be stained with these violets either when fresh or after being hardened in spirit; and the colouring agents have this peculiarity, that certain tissues, as cartilage, decomposes them into a violet-red and a blue-violet, each of which becomes fixed in different elements of the tissue; the hyaline matrix, for example, assuming a red colour, whilst the nuclei and cellules, as well as the cartilaginous capsules, become of a blue-violet tint. The normal tissues of the liver, kidney, and spleen, however, do not decompose the violets, but, when amyloid degeneration is present, the degenerated and semi-transparent parts resembling colloid become of a violet-red, whilst the normal elements are tinted of a violet-blue, and thus a means equal, if not superior, to that of iodine is afforded, by which the changes may be followed.

A Double Weight for Balsam Mounting is described in the 'American Journal of Microscopy' (No. 4), by the Rev. J. L. Zabriskie. He says, in mounting microscopic objects in balsam by the use of the hot-water bath, and a weight upon the covering glass —which process is slow, but sure—one usual difficulty to be overcome is the tendency of the cover to gradually slide away from its original central position. I do not know if this plan has been adopted before;

but I have found the little home-made apparatus here described very satisfactory in conquering this difficulty.

Two bullets are slightly flattened at one point by a few light blows with a hammer, so that they will stand on this flattened part without any tendency to roll. A piece of No. 16 iron wire, half an inch long, and sharpened at one end, is driven into the opposite part of each bullet for a handle. A gash, $\frac{1}{8}$ inch deep, is also cut in the side of each, which may be readily done with a hammer and a stout-bladed pocket-knife. A strip of tin-plate, $\frac{1}{8}$ inch wide and 2 inches long, is cut, and each end of the strip is thrust in the gash in a bullet, where the tin can be firmly secured by hammering the gash together with the sharp edge of a tack-hammer, or other suitable tool. A cube of cork is cemented at the middle of the under side of the tin strip, of such thickness that it will touch the covering glass when in use, and a varied pressure can be obtained from the weight of the bullets by slightly bending the tin strips up or down, before the apparatus is placed in position. Care should be taken that each bullet just touches the glass, in which case the slight springing of the tin strips will act as a lever, with the cork as a fulcrum; and if more pressure is needed, it can be had by placing an additional weight on the cork.

The advantages of this method are, that nothing projects beyond the edges of the glass slip, to take up unnecessary room or receive any accidental disturbing blow; and if the double weight is applied while the slide is cool, and the balsam slightly stiffened, even if the top of the hot-water bath is not perfectly level, the covering glass will be securely kept for any desired time, without the slightest change from its original position.

The best Mode of examining the Umbilical Cord of Mammalia.—Mr. Lawson Tait, who has recently read a very valuable paper on the structure of the cord before the Royal Society,* gives the following as the best mode of preparing specimens. He says:—First of all I may say that I have in no instance drawn a conclusion from observations made on a cord otherwise than perfectly fresh, unless it is distinctly stated to the contrary. I have found the examination and treatment of tissue which has been subjected to hardening reagents so unsatisfactory that I have quite discarded it.

All my sections are made by the freezing process (described in Humphrey and Turner's Journal for May, 1875), so that sections of the perfectly fresh cord of about $\frac{1}{600}$ of an inch in thickness have been examined. These have been subjected to various treatments—as simple clearing by glycerine, destruction by acetic acid, staining by silver lactate, and by my various indifferent staining fluids, hæmatoxylin, litmus, cabbage, &c. (also described in Humphrey and Turner's Journal).

My injecting apparatus is so arranged that it acts automatically when set at work. The tissue injected and the whole apparatus is surrounded by a current of warm water, the temperature of which is registered. The injecting force is supplied by compressed air ad-

* 'Proceedings of the Royal Society,' No. 168.

mitted directly to the surface of the injecting fluid, and the pressure is registered by a manometer. The nozzles used vary in diameter from 1 to 4 millims. The fluid used is a 10 per cent. mixture of Seitels's Berlin blue suspended in firm size. This does not stain the tissue, because it is not in solution, yet its granules are too small to be seen by any power of lens in my possession. That it is not in solution is certain from the fact that it is completely removed from the fluid by adding some albumen and boiling. Similar but not so satisfactory results may be obtained by Davies's granular carmine; but here the granules are too large to enter the canals, save under such pressure as produces frequent extravasation.

One disadvantage of the Berlin blue is that it contains a little free acid, and must do so to remain visible. After a short time this acid destroys the colouring of the stained nuclei; so that, save in an almost perfectly fresh specimen, it is impossible to demonstrate the relations of the nuclei to the canals when distended by the injection.

The method of injection of these canals is apparently very rough. It consists simply in inserting a small nozzle superficially into the substance of the cord over and parallel with the course of a vessel, tying it in, and injecting under a low pressure of 50 or 60 millims. of mercury.

Schweiger-Seidel made the very obvious objection to this (Recklinghausen's) method that any appearances presented by it would be simply those of extravasation. Such was my own belief when I first tried it on the cord; but very short experience showed me that the result was a regular and uniform injection of a system of canals, and that extravasation was very rare and always limited to the immediate neighbourhood of the wound in the cord. With the whole apparatus at a temperature of 47° and a pressure of 60 millims., I have injected a column of the cord for a distance of 9 inches in about half an hour. The injection travels rather more rapidly in the direction from the child to the placenta than in the opposite direction.

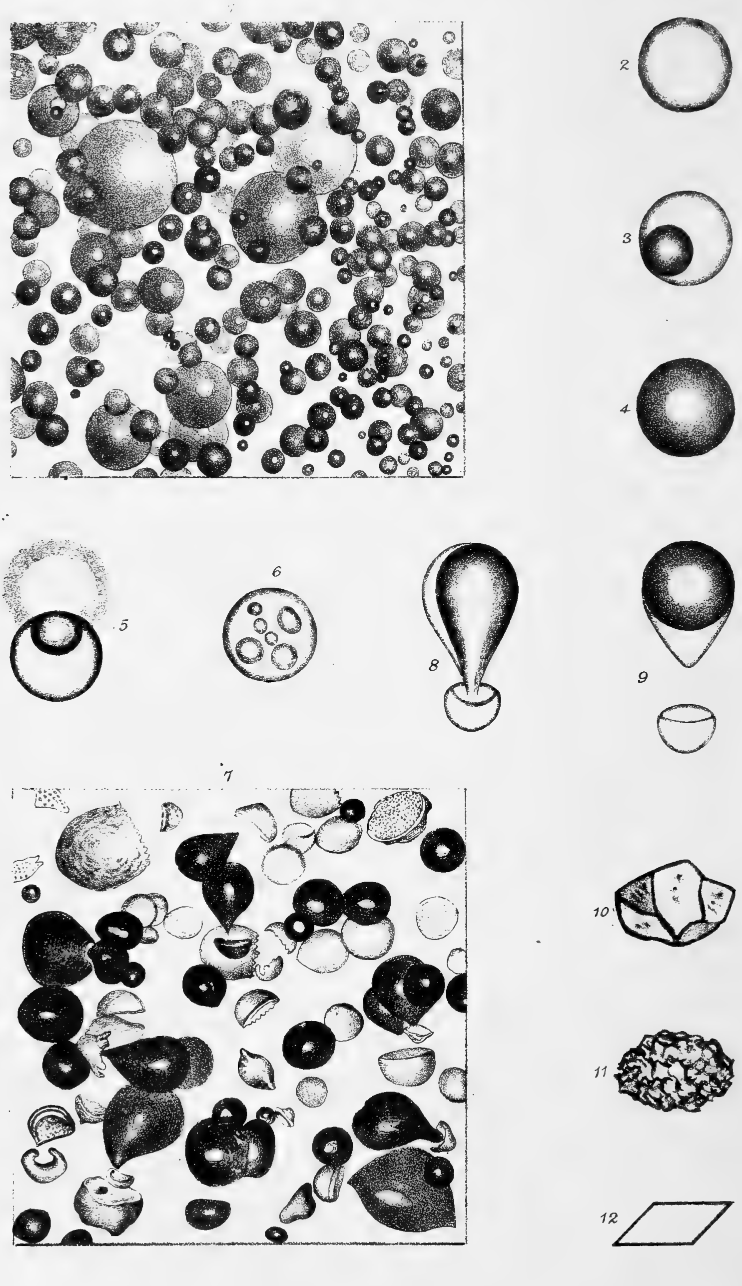
I have repeatedly seen minute streams of the blue injecting fluid flowing from the surface of the cord into the water surrounding it, even at as low a pressure as 55 millims., with a nozzle only 1 millim. in diameter; yet at a pressure of 350 millims. I have not produced a rent in the surface of the cord, though I have produced numerous extravasations into the alveoli and into the neighbouring columns.

American Observations on Cellulose in Blood.—We desire to call attention to the published results of Mr. T. Taylor's (Government Microscopist, U.S.A.) investigations, which we think are likely to bring American science into contempt. He has lately been publishing a series of papers, which are the veriest nonsense in reality, so much so that we are surprised at their appearing as they do in a Government report. In the first place, the author never takes the trouble to inquire what has been done in European countries on the subject, and he plunges into a complex question entirely foreign to his pursuits, and the highest power he has employed is 150 diameters. Why, he could barely see the blood-globules of man with such a power. Will anyone who has ever examined the blood under a high

power read the subjacent lines with approval :—" The fresh blood of a fowl was whisked with a fork to separate the fibrine from the liquid portion. The fibrine was next dissolved in dilute caustic potash, to which was added acetic acid until the precipitate ceased to form ; a portion of it will ultimately float on the top ; remove a portion of it by means of a clean glass rod, and place it on a microscopic slide ; add to it one drop of transparent solution of tincture of iodine, followed immediately by one drop of concentrated muriatic acid ; then examine it carefully under a power of about 150 diameters, for starch, if it is present, will appear in granules of a blue or purple colour. At this stage of the process these chemicals will not convert amylaceous cellulose into starch, even if present. To this same mixture add one drop of concentrated sulphuric acid ; place a glass disk over the contents, and blue amylaceous matters in various forms will probably be found ; but should there be an entire absence of blue colour, and opaque brown particles appear, remove the disk and apply the chemicals again as before. Should too much sulphuric acid be employed, the whole colouring mass will be dissolved. The amylaceous matter present at the same time appears, when superfluous sulphuric acid is used, in white translucent bodies, dissolving in streaks ; but the proper admixture of iodine solution with muriatic and sulphuric acid will give the desired results. Many experiments will need to be made by microscopists before sufficient expertness and satisfactory results can be obtained. That portion of the blood which remains after the fibrine has been removed from it has been examined for starch granules, but none were found ; when tested for amylaceous cellulose a trace of it appeared. I conclude, as a result of hundreds of experiments, that amylaceous cellulose is combined with the fibrine of the blood, arterial and veinous, and may be detected in even a minute portion of it, in the manner described." *

Wythe's Illuminator.—In the 'American Naturalist' (July), Dr. J. H. Wythe recommends for oblique illumination a right-angled prism with a plano-convex lens, cemented to and covering one of its narrow sides, and an ordinary French triplet fastened to the other, close to the farthest angle. Arranged with the plano-convex lens directly downward, the axis of the triplet would be horizontal, and a horizontal cone of achromatic light would be furnished ; while by slightly tilting the apparatus, an available and extremely oblique illumination is obtained.

* 'Monthly Report of the Department of Agriculture,' June, 1876.



W. West & Co. lith.

The Microscopical structure of Amber.

THE

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I.—*On the Microscopical Structure of Amber.*

By H. C. Sorby, F.R.S., &c., and P. J. Butler, F.R.M.S.

(*Read before the* Royal Microscopical Society, *October* 4, 1876.)

Plate CLVIII.

Anyone examining a number of specimens of amber cannot fail to notice that some are more or less clouded, or even quite opaque. This is mainly due to the presence of minute cavities, which in some cases are so numerous that there would be several thousand millions in a cubic inch. Such cavities are comparatively rare in the very transparent specimens, and hence their development cannot be looked upon as having been a condition essential to the change of the original more or less liquid balsam or resin into such a brittle substance as amber. Their formation and character must, however, be fully taken into account in forming any theory to explain the origin of this fossil resin, and the changes which have subsequently occurred.

In order to examine these cavities to advantage, the specimen must be cut into a very thin section, polished on both sides, fixed to glass with Canada balsam, and also covered by a thin glass cemented down with the same material. A moderately high magnifying power should be used. For general examination a $\frac{4}{10}$th is very suitable; but for more detailed study of the individual cavities a $\frac{1}{8}$th is much better.

There is not only a very great variation in the number of the cavities in different specimens, but even in different parts of the same section, and they often occur in irregular bands and streaks, separated by bands containing comparatively few. Large cavities may occasionally be seen with the naked eye, and sometimes they have all the characters of bubbles of air introduced into a soft balsam by the enclosed insects when struggling to get free.

The more common cavities are, however, very small. Those so much as $\frac{1}{1000}$ of an inch in diameter are comparatively rare. From $\frac{1}{2000}$ to $\frac{1}{30000}$ of an inch in diameter is the usual size. Occasionally there are specimens in which they are so minute that they cannot be accurately defined, and appear to be not more than $\frac{1}{100000}$ of an inch in diameter.

Having now given a general account of the manner of their occurrence, we proceed to consider the detailed characters of these cavities. One of the first things which strikes an observer accustomed to study the cavities in minerals is that those in amber are usually of almost spherical form. This fact, and the general manner in which they are associated where numerous, will be seen by reference to Fig. 1, Pl. CLVIII. This contrast between the form of the cavities in amber and of those in crystals is easily explained. Those in crystals are usually vacant spaces left during the growth of the crystal, and very often are characterized by being bounded by crystalline planes, and not unfrequently they have exactly the same form as minute crystals of the substance in which they occur. Thus, for example, in common salt they are often rectangular, and in quartz sometimes six-sided, with pyramidal ends. On the whole, we may say that the shape of the cavities in crystals, to a very great extent, depends on the crystallization of the substance enclosing them. On the contrary, the cavities in amber have a form quite independent of any structural direction, and are like the minute bubbles in a stiff liquid, which naturally assume a spherical shape, because a sphere has the largest possible volume in proportion to the area of the surface. Though this is the general character of the cavities in amber, yet there are some very striking and interesting exceptions, as shown by Fig. 7; but both the normal and the abnormal forms combine to prove that the amber was originally a stiff liquid, occasionally subjected to internal movements, which disturbed the naturally spherical shape of the cavities.

In studying the cavities in detail, it will be convenient to divide them into three principal groups, as follows:

1. Cavities filled with liquid.
2. Cavities containing both a liquid and gas.
3. Cavities filled with gas.

There is no practical difficulty in distinguishing these one from the other. The refractive power of the liquid is so little less than that of the amber itself, that, though the cavities are very well marked by a dark outline, it is comparatively narrow, and when illuminated by a condenser with an aperture of considerable angle and a wide opening underneath it, light can be seen passing through by far the larger part of the entire area, as shown by Fig. 2. When a cavity is partially filled with liquid, this same character is maintained over a portion of the area; but in addition we see a bubble, characterized by a dark outline, which is nearly as broad as one-third of the diameter of the whole bubble, as shown by Fig. 3. With the same illumination, those cavities which are entirely filled with gas are of course like bubbles, extending over the entire area of the cavity, and the breadth of their dark outline is about one-third of the entire diameter, and

light is transmitted through only a comparatively small central circular spot, as shown by Fig. 4, which is nothing more than the image of the opening under the condenser seen out of focus.

Except in a very few cases, there is no difficulty in distinguishing these various cavities from one another, but still there are cases in which an incautious observer might be led into error. Those filled with liquid act like small lenses, and at certain adjustment of the focus the images of other cavities lying below them can be seen, and often look extremely like enclosed bubbles. The true nature of the case may, however, be generally ascertained by a little attention to detail. Thus, for example, Fig. 5 shows what might easily be mistaken for an enclosed bubble, but when the focus is adjusted to see it distinctly the somewhat obscure outline of another cavity, at a lower level, is visible, and by altering the focus there is no difficulty in proving that the apparently enclosed bubble is only the image of the lower-lying cavity. In some cases we may see as it were many bubbles, as shown by Fig. 6, due to the images of several subjacent cavities. If such a deceptive appearance were mistaken for a cavity containing several bubbles, an observer might be led to conclude most erroneously that the cavity was filled with some substance now no longer liquid, since the permanent existence of more than one bubble is impossible in fluid cavities, and only met with in glass cavities, as described and figured in Mr. Sorby's paper on the microscopical structure of crystals.*

Though it is almost certain that the liquid in the cavities of amber is water, yet it cannot be said to be completely proved by any facts hitherto observed. In the case of anhydrous minerals, like quartz, the water can be expelled by heating in a glass tube, and condensed as hoar-frost in another part of the tube surrounded by a freezing mixture, and proved to be water by thawing at 0° C. This method, however, cannot be applied in studying amber, since on heating portions devoid of fluid cavities it is decomposed, and amongst other products yields water.

It appears to be almost certain that a very large proportion of the cavities, like Fig. 4, which may be said to be empty, are only fluid cavities which have lost all their liquid. On examining some sections of amber it is easy to see that in the centre of the thickness of the slice nearly all the cavities are full of liquid, and by gradually changing the focus so as to examine the cavities nearer the surface, there is a larger and larger proportion of the empty, until near to the surface none retain any liquid. In such a case it appears almost certain that the liquid has been lost during or since the preparation of the section. But independent of this there appears to be good evidence to prove that the liquid has also sometimes been lost from the exterior part of the amber, before being cut into a thin slice,

* 'Quarterly Journal of the Geological Society,' 1858, vol. xiv. p. 453.

since near what was the surface all the cavities are empty, even in the centre of the thickness of the section. This loss of liquid is also to some extent dependent on the total relative bulk of the cavities, since, as might be expected, the solid amber has presented a greater obstacle to evaporation than that rendered comparatively porous by the enormous number of vacant spaces often met with in opaque bands. There is thus no difficulty in understanding why so many thin sections of amber show only empty cavities, as to have led some observers to conclude that this was their original normal condition, and to explain their formation by assuming that a gas had been evolved in the interior. Probably this has really occurred to some extent, but still the evidence is not perfectly conclusive.

In the centre of the thickness of those sections in which the cavities retain their liquid, and surrounded by many cavities still quite full, a few empty cavities may be seen, and it seems difficult to understand how they and they alone can have lost their liquid, and it is far more probable that they were originally filled with a gas.

The liberation of a gas in the midst of the amber appears to be established by the characters of such cavities as Figs. 8 and 9, which, however, are comparatively rare. Those like Fig. 8 at once remind an observer of a balloon with an attached car. By carefully looking over many sections we have been able to find only one or two cavities more or less closely approximating to this form that were entirely filled with liquid. They are usually partly filled with liquid, and partly with gas, as shown by Fig. 8, and usually, if not always, the car, so to speak, is full of liquid, and the balloon itself mainly or entirely filled with gas. Such cavities are most eminently characteristic of amber, and totally unlike any seen in crystals. Their formation appears to have been brought about in the following manner. What we may call the car was originally a spherical fluid-cavity of the usual kind, and after the amber immediately surrounding it had become somewhat hardened, a gas was given off, either by some chemical change or by the pressure being relieved by a contraction of the general mass of the amber after its exterior had become somewhat hard. If the surface of the original cavity had been equally soft in every direction, the evolution of the gas would have merely dilated the surrounding amber, and given rise to a somewhat larger spherical cavity, partly filled with gas, like Fig. 3; but if one side of the original cavity could yield much more readily than the rest, the gas would be able to force its way out into the surrounding softer resin. The actual or relative pressure being thus relieved, the softer side of the original spherical cavity partially collapsed, and some of the liquid rose up from the car into the balloon. By these simple suppositions all the peculiarities of

such cavities as Fig. 8 may be explained. In many cases, however, the changes did not end at this stage, but internal movements in the mass of the amber detached the car from the balloon, and separated them, as shown by Fig. 9. These facts manifestly throw great light on the character of the changes that took place when the original soft resin was converted into amber. Some cavities have also been much distorted and drawn out by a general change in the dimensions of the amber, like the bubbles in glass drawn out when melted.

On examining with polarized light a section of amber cut through the centre and exterior crust, it may be seen that the central part has no depolarizing action, and has the same optical properties as annealed glass. The exterior skin has, however, a well-marked depolarizing action, and by carefully studying the character of the double refraction, it is found to be such as would result from a relative increase in the bulk of the exterior crust or contraction of the internal mass, that is to say, the depolarizing action is what would be caused by a pressure acting in the line of the circumference, and not in the line of the radius. This is in complete accordance with the facts indicated by the balloon-shaped cavities. At one time we thought that a similar depolarizing action was met with round some of the cavities, giving rise to black crosses, as described by Sir David Brewster.* He attributed this depolarizing action to pressure exerted by an enclosed gas tending to enlarge the cavity; but the character of the double refraction indicates a pressure tending not to *increase*, but to *diminish*, the size of the cavities. It is the reverse of that met with round minute crystals enclosed in diamonds, as described in our paper on the microscopical structure of some precious stones,† which is just what would result from a contraction of the mass of the diamond surrounding the enclosed unyielding crystals. Sir David Brewster did in fact admit to us that he had in some way or other been misled in concluding that the cavities in amber indicate a pressure exerted from within. We now very much question the propriety of associating the black crosses with cavities as such.

By far the larger number give no trace of black crosses with polarized light, and on the whole the depolarizing action must, we think, be attributed to some cause that is independent of the presence of any elastic gas.

In carefully looking over different parts of various sections of amber under polarized light, we found that the black crosses were by no means uniformly distributed, but occurred in groups, which appeared to be to some extent determined by the existence of a more general depolarizing action, like that met with in the

* 'Transactions of the Geological Society,' vol. iii. p. 455.

† 'Proceedings of the Royal Society,' 1869, vol. xvii. p. 291.

external crust. Anyone using only a moderately high power, and not paying attention to minute detail, might easily be led to conclude that the centres of the black crosses were always cavities; but by using a high power with excellent definition, it may be seen that they more commonly occur round more or less irregular and angular particles, like Fig. 10. As far as mere shape is concerned, these particles are like minute grains of sand. Some, indeed, are of more regular shape than that figured, so much so that it is occasionally difficult to be seen that they are not really cavities filled with liquid; and in fact there is good reason to believe that genuine full or empty cavities do really exist in the centre of black crosses. On the whole, therefore, it appears as though the production of the depolarizing action of the surrounding amber cannot be attributed to the outward pressure of either cavities or solid fragments. As far as the optical characters are concerned, it is rather such as would result from a tension in the line of the radius. This, however, is quite impossible in the case of a cavity, and all but impossible even in the case of an enclosed fragment. We are therefore compelled to conclude that the depolarizing action of the surrounding amber is due to the same cause as that met with in the external crust. This would agree perfectly well with the fact of the black crosses being more commonly associated with a certain amount of general depolarization. Taking everything into consideration, it appears as though, whilst the general mass of the resin was still somewhat plastic, a change took place on the external surface and round some enclosed angular particles and cavities, which made the material relatively so hard that its molecular state could be permanently affected by mechanical pressure. The more rapid hardening of the exterior than of the interior is what would most certainly occur in the case of any soft balsam exposed to the air; and an increased tendency to the same change round enclosed angular fragments is quite in accordance with what is so often observed in the deposition of crystals, and with what so often happens where a condition of unstable chemical equilibrium is upset by mechanical means. It must be borne in mind that in the optical peculiarities due to tension or pressure, we very generally have evidence rather of *relative* than of positive changes of dimension, and that in the case now under consideration the result may have been brought about not by an increase in the bulk of the material on the surface or round the fragments, but by a decrease in the volume of the rest of the mass. This latter supposition would agree with the facts already described when explaining the peculiarities of the balloon-shaped cavities.

Besides such angular particles as shown by Fig. 10, others occur of much more complex character, and of doubtful nature, like Fig. 11. The examination of this led us to wonder whether it

could be some kind of minute spore; but the evidence for or against such an opinion is defective. Sometimes minute crystals are seen like Fig. 12, having well-marked angles, and a strong depolarizing action. These must not be confounded with crystals on the surface of sections covered with glass cemented by balsam, which have perhaps been formed *in situ* by the mutual action of the balsam and the amber.

General Conclusions.

It will thus be seen that amber furnishes us with an excellent illustration of the important facts that may be learned by carefully studying the minute cavities in minerals. All the circumstances connected with the occurrence of amber prove that it cannot have been formed in anything like the same manner as the crystalline minerals in igneous or stratified rocks. It is interesting to find that the general principles of research adopted in Mr. Sorby's paper on the microscopical structure of crystals are equally applicable in the case of a substance of such an entirely different nature as amber.

Studied in the same manner as has led to such satisfactory results in the case of minerals and rocks, amber leads to very different but yet equally satisfactory conclusions, which may be summed up as follows:—It was originally a soft balsam, which gradually passed into a hard resin. During this change water was eliminated, which was often retained in the interior as minute globules, constituting the fluid cavities; and gas was also evolved, often giving rise to the gas cavities. These various cavities were normally spherical, but have sometimes been disturbed by general or partial internal movements of the resin whilst it was still more or less plastic. The volume of the resin itself was also diminished in such a manner as to give rise to internal tension, which has modified the optical characters of the separate pieces.

II.—*Diatomaceæ in Slides of Santa Monica Deposit.*

By F. Kitton, Hon. F.R.M.S.

(*Read before the* Royal Microscopical Society, *October* 4, 1876.)

The Santa Monica Diatom Deposit.

A sample of this deposit was presented to the Society by H. Hanks, Esq., of San Francisco, and being examined by F. Kitton, Esq., F.R.M.S., was reported upon by him as follows:

No. 1. *Eupodiscus Rogersii.*—This specimen shows the "furrow" much more distinctly than usual; it is, however, present in the fossil forms from the New Nottingham (Bermuda) deposit. A careful examination of a series of valves of *E. Argus* * shows that the "furrow," although much less marked than in the preceding species, is present also in that form. If the "furrow" is of any generic value the above-named forms should be referred to the Aulacodisci. This species is rare in the Santa Monica deposit.

No. 2. This slide contains the following forms: Stictodiscus Californicus, three valves; Arachnoidiscus Ehrenbergii, three valves; Actinoptychus, sp.? one valve; Triceratium Arcticum, three and four angled; Raphoneis, sp.? one valve; Auliscus Hardmanianus, one valve; Coscinodiscus radiatus (according to Schmidt this is C. radiatus), var. minor of Ehrenberg; Navicula excavata, Schmidt. ? var.

No. 3. Aulacodiscus pulcher, one valve; Arachnoidiscus Ehrenbergii, four valves; Stictodiscus Californicus, three valves; Coscinodiscus radiatus, as above; Actinoptychus Gründleri, Schmidt (very fine and perfect); Auliscus reticulatus ?; Navicula prætexta.

No. 4. Arachnoidiscus and Stictodiscus, as in preceding slides; Navicula prætexta; Asteromphalus variabilis.

No. 5. Arachnoidiscus and Stictodiscus, as before; Triceratium Montereyii; Actinoptychus Gründleri; Aulacodiscus pulcher; Actinocyclus interpunctatus; Cocconeis splendida; Navicula spectabilis.

No. 6. Actinoptychus, probably a new species.

No. 7. Auliscus Hardmanianus, perfect valves.

No. 8. Eupodiscus oculatus.

No. 9. Cyclotella operculata, var. perhaps identical with Ehrenberg's Melosira sol (figured in the 'Microgeologie').

I did not find in the Sta. Monica deposit sent me any new species excepting No. 6. A larger quantity might perhaps yield some new ones.

No. 9 is from the Madrid (?) deposit sent to me for examination by the Society.

* *E. Argus* has similar markings, and is perhaps only a variety of *E. Rogersii.*

III.—*A Curious Fact in connection with certain Cells in the Leaves of Hypericum Androsæmum.* By W. Hinds, M.D.

(*Read before the* Royal Microscopical Society, *October* 4, 1876.)

The presence of motile protophytes in certain cells of the leaves of Tutson certainly struck me by surprise. It was a circumstance I was not prepared for, and I shall be glad if any investigator who may have met with this will kindly give his opinion as to the conditions of their production.

The specimens of the plant examined were brought to me, amongst others, from near Tunbridge. On examining the leaves, I found the true glandular dots, as it appeared to me, almost entirely absent; but here and there, often near the margin of some leaves, were minute light-coloured puncta with no chlorophyll. In these specimens the puncta were mostly seen in the centre of the small sections of parenchyma, surrounded by the ultimate ramifications of the vascular system of the leaf. Examining one of these somewhat angular spaces in a thin slice of epidermis and subjacent from the upper side of the leaf, the central light punctum was seen to be replete with very active motile bodies. They were not of uniform size, but varied considerably. The examination was made with a $\frac{1}{4}$ inch objective, and afterwards by $\frac{1}{16}$th immersion.

The leaves having been afterwards kept in a vasculum for one week, these motile bodies were still seen, but in a somewhat less active state and in diminished numbers.

Since the above notes were written, I have examined leaves of a closely allied* species growing in this locality (Birmingham), and I find just the same conditions present. The clear spaces appear to be complete colonies of motile forms, with some still forms, resembling the active. What do these conditions indicate?

As to the nature of the spaces, no doubt they are translucent from the absence of chlorophyll. The cellular is not the same as in the other parts of the angular space. The cells are somewhat larger, less regular in outline, and less angular than those which contain the chlorophyll. These translucent spaces being often in the centre of the angular space have no direct connection with the vascular tracks which constitute the ultimate capillaries of the leaf. They are, therefore, in such cases farthest removed from the immediate source of vascular nutrition. Their vital powers may be probably thus lowered, and this may have some relation with the presence of the motile particles described.

Another supposition might be advanced. Are these dots truly glandular? If so, do they secrete a substance liable to fermentation

* *H. Calycinum.*

during the life of the plant? In the latter case we might have bacteria produced, or some forms of minute active vegetable life, as a result. What is the nature of the motile bodies themselves? The history of the bacterium, with its variable forms and conditions, is still left somewhat in obscurity. Zoospores have been known to develop within lichen fungi; is it impossible they could develop in the locality mentioned?

Are they protococci in the active state? Without further investigation, I will not dogmatize on the matter, but content myself with the mere statement of fact.

IV.—*The Present Limits of Vision. Further Elucidations.*

By Dr. Royston-Pigott, F.R.S., &c.

(*Taken as read before the* Royal Microscopical Society, *October* 4, 1876.)

It has occurred to me that perhaps several of the readers who are unacquainted with my methods may find a few explanations not devoid of interest, especially those who wish to repeat the experiments for themselves.

I therefore now propose to say a few words:

(1) On my method of producing vivid miniature disks of the sun.

(2) On measuring them theoretically.

(3) On the application of the undulatory theory of optics to the calculation of the diameters of the enlarged spurious disk, and the breadth and size of the primary black ring and of the successive bright and dark rings of the diffraction.

1. *Vivid Solar Miniature Disks, and how to get them.*

Arrange an Amici prism in the sunshine so that by total reflexion you can see an aerial brilliant image of the sun; mine has an imbedded lens, giving a focal length of three inches. In front of this I place between the prism and the eye a $\frac{1}{4}$ objective for the telescope; but for the microscope I place the prism 100 inches distant, and fix a fine $\frac{1}{8}$th as condenser, to form the miniature in the focal plane of the microscope, without the use of any silvered mirror, which utterly spoils these charming phenomena. Viewing it with a fine $\frac{1}{8}$th and a C eye-piece, by lengthening the tube the overpowering effulgence may be subdued so as to render inspection tolerable; but no blue shade or coloured glass must be employed.

I am glad to be able to state that a power of 1000 subdues the sunlight sufficiently without coloured glass; but a little care is required in selecting the eye lenses, so that the field of view may just embrace the whole spectacle. To those who have never seen these glorious displays, I may strongly recommend this interesting department of microscopical optics, as at once delightful and recondite, yet most instructive.

The telescope, with a power of 300 diameters at a distance of 100 feet, gives, up to a certain point, almost identical phenomena for axial displacements. You are presented with the same richly enamelled patterns of overpowering brilliance and exquisite forms.

Whether, therefore, the telescope or the microscope be used, the same results follow within the same limits of aperture.

Now for the question of *destruction of diffraction.* This term is used to denote the effect of undulations passing through apertures or gratings. Interferences result which are more or less conspicuous according to the intensity of light. *But with mild or diffused light they are almost totally invisible.*

I beg to direct the attention of the Society to the drawings, which feebly indeed and only very imperfectly display the phenomena, and which I have sent to the Secretary. The colours of the rainbow, beautiful as they are, entirely pale before the variegated and vivid intensity of solar miniatures thus examined.

Fig. 1 represents the miniature of the house H, seen by the telescope in absence of sunshine. So soon as the sun comes out, the diffraction phenomena obliterate all the details of the house, so beautifully distinct before, as seen in Fig. 2.

If now a miniature reflexion of the sun be arranged at the front door of the house, the instant the sun shines a similar effect is produced. Suppose, now, the condenser of a microscope be a fine $\frac{1}{8}$th, and the house be very much nearer, the miniature house, if white, can be beautifully examined by a good microscope; but the instant the sun shines, the diffraction phenomena completely expunge the details before visible.

So, whether considered telescopically or microscopically, destruction of bright diffraction reveals new details invisible before.

According to the brightness of the disk do the number of rings increase. I have seen eleven with the telescope, and above twice as many with the microscope. In the latter instrument they appear at almost exactly equal spaces apart, and gradually merge into coloured indistinctness. Whether the one or the other instrument be used, very perfect centering of the glasses can alone display the beauty and perfect truth of the deep jet-black tiny ring immediately surrounding the primary disk. The intensity of this blackness, holding its own sharp form in the midst of a painfully bright effulgence, is perhaps the most marvellous of all the phenomena of the undulatory theory, and exactly fulfils the predicted condition of the intercussating waves of the vibrating particles of the ether.

The microscope has this enormous advantage, it is independent of external wind. I found the prism placed on a table when the sun was shining through an ordinary glass pane, developed all the phenomena with the utmost certainty. Not so the telescope. The air is seldom fit for these delicate observations, except at about 7 A.M. in spring time. Radiation is too abundant, irregular currents abound, the forms of the disks are marred, and the diffraction lines become capricious and flickering.

But once only have I ever observed the dark rings of the double star Castor in perfection. It was spring; a London fog embrowned the sky as twilight advanced; a still haze overspread the whole landscape; the moon showed an absolutely steady atmosphere: it bore 500 with perfectly quiet definition; the huge boulders, profusely spread along the Apennine slopes, glittered distinct, and the shadows of caverns and mountains appeared as black as ink. Turning my $5\frac{1}{4}$ Wray equatorial on Castor with 500, each star of the double was surrounded with a perfectly depicted jet-black

ring, as sharp and true as any draughtsman could draw them with indian ink; but no other rings, either bright or dark, were seen; all were destroyed by the concealing haze. Such fascinating beauty of definition in this country is rare indeed.

I have attempted to represent this effect in Fig. 3, in which the dark ring was about the one-fifth of the larger disk.

By imitating the haze we may thus similarly conceal or destroy the outer diffractions. Thus the subdued light of the northern or north-western sky at sunset, or a green sunset sky in the west, portending rain, as people say, may develope marvellous powers of definition by reducing the errors of diffraction also. Daylight is always preferable to lamplight, and by shading the eye properly, much can be then seen which is hidden by artificial glare. I believe that many excellent microscopists infinitely prefer daylight when sufficiently bright. It is, however, astonishing how much the eye gets accustomed to a dim light by promoting the expansion of the pupil of the eye. There is no doubt daylight tires less, and therefore injures less in the long run than lamp illumination.

2. *The Method of Measuring the Diameter of Solar Disks Trigonometrically.*

When rays of light pass through an object-glass centrically (as they then pass without deviation), they preserve their original direction, and the image and the object both form the same angle at the centre of the object-glass. If, therefore, F be the focal length, and 32 minutes of arc be the angle subtended by the sun—

$$\text{Diameter of sun's image} = F \sin. 32'.$$

And then we have only to multiply the focal length of the lens in question by the natural sine of the sun's diameter at the time, and the diameter of the true disk in miniature is obtained.*

The sun's diameter varies, however, somewhat:

In January it is	$32\frac{1}{2}'$	.. .. ..	sine = ·00945 = $\frac{1}{106}$.
„ April „	$32'$	.. .. ..	sine = ·00931 = $\frac{1}{108}$.
„ July „	$31\frac{1}{2}'$	.. .. ..	sine = ·00916 = $\frac{1}{109}$.

Hence the diameter of a miniature of the sun formed by a 1-inch lens will be respectively, in those months:

$$\tfrac{1}{106}, \ \tfrac{1}{108}, \ \tfrac{1}{109} \text{ inch nearly.}$$

And this, reduced by a 3-inch lens instead of a 1-inch, would be:

$$\tfrac{3}{106}, \ \tfrac{3}{108}, \text{ and } \tfrac{3}{109}.$$

---

* The diameter of a white wafer on a black-board placed at such a distance as to subtend the same angle as the sun at the time would be 1 inch placed at 106 inches distance.

Still further reduced by a $\frac{1}{4}$, placed before the prism, would be:

$$\tfrac{1}{142}, \quad \tfrac{1}{144}, \quad \tfrac{1}{146}.$$

Supposing, now, the prism to be 100 inches from the $\frac{1}{8}$ condenser, the miniature would be again reduced 800 times, and the size of the miniatures in January, April, and July should be, in fractions of an inch:

$$\tfrac{1}{113000}, \quad \tfrac{1}{115000}, \quad \text{and} \quad \tfrac{1}{117000}.^*$$

But if the actual disk be measured with a delicate eye-piece, screw-micrometer and spider lines, the disk in May measured $\frac{1}{16000}$ of an inch; and the black ring nearly the $\frac{1}{50000}$, as nearly as the extreme brightness permitted of such estimation.

Roughly, then, it may be stated that generally the solar disk formed by a lens should be one-hundredth part of the focal length; for a 1-inch lens gives an average of $\frac{1}{108}$ inch in the mean value.

Doubtless the moon can be examined microscopically in a similar way, and its diameter by a 1-inch lens would also nearly equal

$$\tfrac{1}{115} \text{ inch.}$$

And some persons have actually turned the microscope into a weak telescope.

3. *The Practical Application of the Wave Theory to the Calculation of the Black and White Diffraction Rings and the Central Disks consequent upon the Vibration of the Particles of Ether and the Interference of Undulating Waves.*

The expression for the disturbance of an ethereal particle at a given point and instant of time is composed of two factors; the first expressing the intensity of the brightness, or showing darkness; and the second factor involves the distance of the origin of light, the aperture, the velocity of the wave, its length, and the epoch.†

* A most remarkable effect of diffraction is observed here. If you enlarge the disk by reducing it less at a certain point, no diffraction rings are developed; but however much you reduce the disk, the spurious disk remains of the same size, but the rings become fainter and fainter. The disk is really shaded off towards the perimeter, and hence as the light fades more and more the disk also begins to diminish, for it is really brightest at its central part.

† Sir G. B. Airy, in his 'Treatise on the Undulatory Theory of Optics,' obtains the following expression for the disturbance:

$$a \sin. \left\{ \frac{2\pi}{\lambda} \left(v\,t + \frac{r\,x}{f} \right) - \mathrm{B} \right\}.$$

Professor Helmholtz gives a similar formula:

$$\mathrm{A} \sin. \left\{ \frac{2\pi}{\lambda} (a\,c + d\,b - a\,t) + \text{constant} \right\}.$$

Sir G. Airy deduces for mean rays where the wave-length is taken $= \lambda =$ ·0000022 inch for an aperture of telescope $2e$, e being the radius of the object-glass, and s the angular semi-diameter of the given ring, or disk, viewed from the object-glass —the following values for $e s$:

$$e \times s = 3{\cdot}70'', 6{\cdot}09'', 8{\cdot}40'', \text{ for bright rings.}$$
$$e \times s = 2{\cdot}76'', 5{\cdot}16'', 7{\cdot}32'', \text{ for black rings.}$$

And for extreme diameter of disk, where it melts into the first black ring, $e s = 2{\cdot}76''$. From which it will be readily seen that for a lens whose aperture is 2 inches, $e = 1$, and $2s$ the diameter in seconds of these rings and disks, will be for a *point* of light:

Diameter of bright rings	7·40″; 12·18″; 16·80″.
Diameter of dark rings	5·52″; 10·32″; 14·64″.
Extreme diameter of disk	5·52″.

[I have found a lens of 1 inch focus and $\frac{1}{2}$ inch linear aperture most convenient to force microscopic miniatures of the distant solar disk 100 inches away, as described above.]

And since these quantities vary inversely as the size of the aperture, for a $5\frac{1}{4}$ achromatic, the actual diameter of the disk measured up to the inner edge of the first black ring would be as $5\frac{1}{4}$ to 2, i. e. $5 \cdot 52''$ would become

$$2'' \text{ and } \tfrac{1}{10}\text{th.}$$

The breadth or thickness of the first black ring will be half the difference between the diameter of the disk and the diameter of the first bright ring:

$$\begin{array}{r l} & 7{\cdot}40 \\ & 5{\cdot}52 \\ \hline 2)\!\!\! & 1{\cdot}88 \\ \hline & 0{\cdot}94 \text{ second per 2-inch.} \end{array}$$

and therefore 0·36 second for $5\frac{1}{4}$-inch achromatic.

The exquisite black rings, therefore, seen by me round the double stars forming Castor, were actually rather less than the one-third of a second. The shading off, however, of the disk at its edge, gradually darkening into the black ring, makes this ring appear somewhat broader than the theoretical value above obtained, and may be taken approximately as half a second of arc with a $5\frac{1}{4}$-inch achromatic linear aperture. A half-inch aperture would give a disk and rings above ten times larger, and more fitted for the microscope.

V.—*Comparative Photographs of Blood-disks.*

By G. Gulliver, F.R.C.S., F.R.S.

My note on this subject, in a late number of the 'Monthly Microscopical Journal,' appears to have produced an impression that I was pretending to decide the relative claims of different American microscopists to the first invention or preparation of the specimens therein noticed. Whereas nothing was farther from my intention, which was simply to give such a notice of this kind of American work as might be interesting in Europe; for, as to the question of priority, I never then had a thought or any precise knowledge, nor can I now attempt to judge of a subject which could be better decided in America than here. But it is due to Dr. J. G. Richardson to state that his photograph, showing in one field of vision the blood-disks of man and the pig, was the first of the kind seen by me, and I suspect was originally noticed by the Editor of the 'Monthly Microscopical Journal' some months ago. Since then I have seen similar photographs, by Professor Thomas G. Wormley, of the blood-disks of the cat and human subject. And in the 'American Naturalist,' May 1876, there appeared a short editorial report of Dr. Richardson's comparative photographs "to illustrate in criminal cases the distinguishable appearances of different kinds of blood." In the same journal, Dr. C. Leo Mees is said to have obtained exquisite results by a modification of Dr. Richardson's process; and this statement is justified by two slides prepared by Dr. Mees, and now in my possession, of the blood-disks of man and the dog. On these slides, and still more on those prepared by Professor Wormley, perfectly circular corpuscles occur, just touching each other, so as to form rows or chains, and thus to afford good opportunities of deducing the mean size of the corpuscles by one measurement of a given number of them. But, as I have long since shown, the corpuscles in one species of the vertebrate class, as seen in a single individual thereof, vary so much in size that their average dimensions cannot be determined with absolute precision; and were this fact kept in view, much needless discussion might be spared. And as to the medico-legal question, probably neither Dr. Richardson nor any other physiologist will affirm that there are not several mammals the blood-disks of which cannot be distinguished from those of man; while there are many others in which the difference is plainly demonstrable, and this notwithstanding variations in the size of the corpuscles of this or that species. And hence, in spite of disturbing circumstances, the diagnosis between the human blood and that of many animals, such as most ruminants and some feræ, is always quite easy, while the corpuscles retain their regular appearance. But how far this is the case in blood-spots, such as come before the criminal tribunals, has to be

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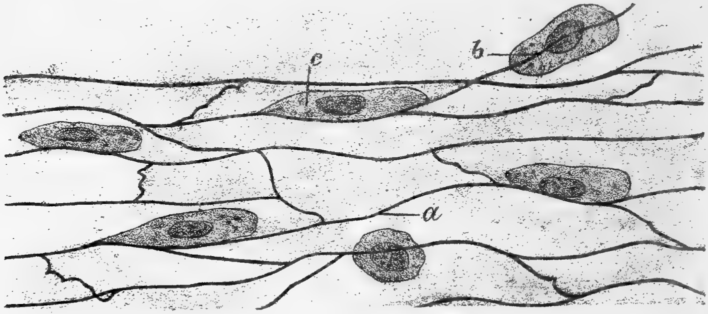

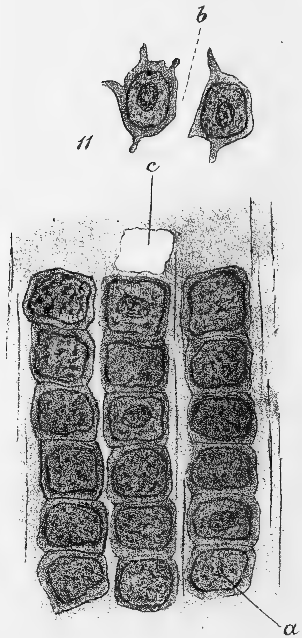

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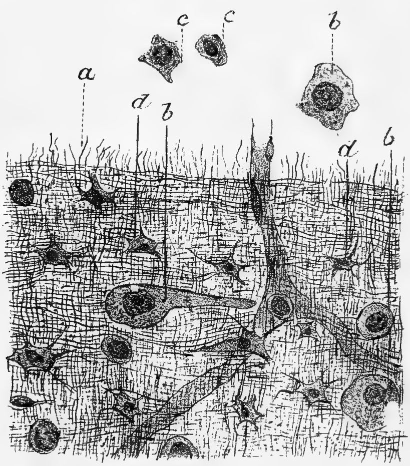
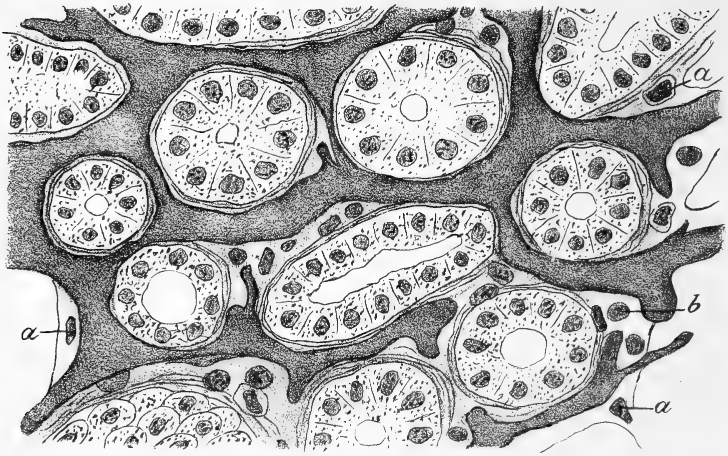


Structure of connective substances

W. West & Co. lith.

9



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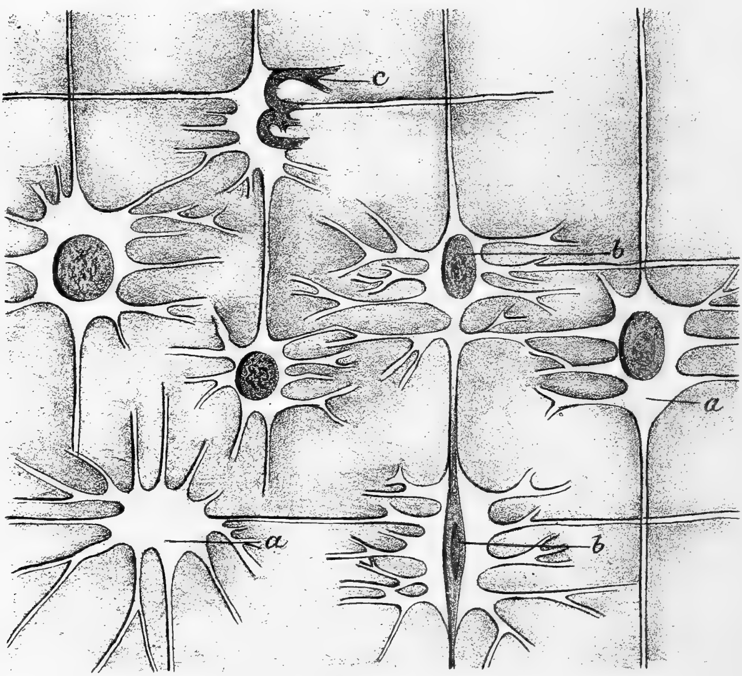


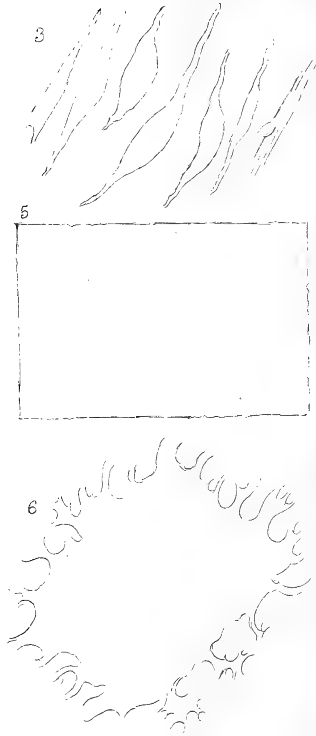
Structure of connective substances

W. West & Co. lith.

To face page 241.

Pl. CLVI.

Pl. CLVII



determined by such researches as Dr. Richardson has so well undertaken; and surely even this attempt, whatever may be the ultimate result, is so praiseworthy as to deserve cordial assistance. Photographs, according to his practice, will be quite essential in this kind of evidence. And those prepared by him, and by Dr. Mees and Professor Wormley, may be accepted as very favourable examples of American micrography. I have yet seen no others of the same kind. Whether they have been followed in England or elsewhere I know not.

VI.—*On the Structure and Development of Connective Substances.**

By Thomas E. Satterthwaite, M.D., Microscopist to St. Luke's Hospital (N.Y., U.S.A.).

Plates CLIX. and CLX.

(*Continued from page* 199.)

4. *Neuroglia,* or *Bind-web,* Seguin (Fig. 7, Plate CLIX.).—But a short time since it was not known positively whether the delicate cementing substance of the nervous system, but more especially of the brain, was granular or fibrous. Even after Virchow claimed that this substance was like the other tissues known as connective, doubt was still thrown upon the matter, for the defining power of the objectives then used was often insufficient to make out these delicate objects. At the present time the actual existence of such a delicate network is hardly called in question, for it may be demonstrated with really good glasses, such as some of the immersion lenses (No. 10) of Hartnack's system. As to the question of the corpuscular elements there is, even now, some question, and it can hardly be regarded that their exact form and shape have been definitely agreed upon by histologists. We find, it is true, that, where there is considerable development of connective material along the central canal of the spinal cord, there we have the ordinary fibres and corpuscles already described, and so, too, near the surface of the convolutions. When, however, we examine the supporting substance of the white and grey masses, it is more doubtful as to the character of the delicate tissue we meet with. The real condition may be tolerably well seen by adopting the following plan. Place any portions of the brain or cord in a weak solution of bichromate of potash (5 per cent.) or Müller's fluid for a few days, and then immerse it in alcohol until hard, and make thin

* Through an omission on the part of the engraver—during the Editor's absence—the figures were not placed on the plates in the first part of Dr. Satterthwaite's paper. Hence the accompanying plan is now supplied, by means of which the reader will be enabled to refer to the plates without difficulty.

sections, which stain in the following solution of hæmatoxylin for twenty-four hours:

Hæmatoxylin	gr. lii.
Aluminis	ʒj.
Aquæ	℥ viij.

M. and strain.

Wash in distilled water and mount in glycerine, tease with needles and examine with a high power; there will then be no difficulty in seeing that the delicate supporting substance of both grey and white matter consists of fibres. They may even be distinctly isolated, for the colouring matter darkens them somewhat, and they become hardened at the same time, so as to be somewhat stiff and unyielding. Then it will be seen that many fibrils are disposed in parallel rows which, perhaps, can hardly be called bundles, but rather thin laminæ; other similar fibrils cross them at various angles, giving to the whole, with a moderately high power, the appearance of a very delicate meshwork (*a*). It does not appear as if the fibrillæ anastomose with one another, though this point would be extremely difficult to settle. It must be stated that possibly some of these fibrils may be nerve elements, though this seems doubtful, because they do not even seem to be connected with the nerve fibres that are distinctly shown by this method of preparation. In the drawing they are not represented, to avoid confusion. Of course, granular appearances are always noted in the brain, but this must be the case when cross sections are made of the delicate fibrillæ. Three are three kinds of corpuscles met with in the brain and medulla. The first are the variously shaped ganglionic corpuscles or cells (*b b b*), then the ordinary lymphoid cells (*c c*), which here are generally seen to have a pale envelope about them; lastly, smaller corpuscles (*d d*) of irregular shapes, and many of them undoubtedly flattened and appearing to have branching processes (*d*). They may be found in considerable numbers, and can be isolated, so that there is no doubt that they exist. The fibrillæ of the neuroglia do not differ substantially in size from the fibrillæ of fibrous tissues elsewhere.

5. *Tendon Tissue* (Fig. 1, Plate CLVI. [Oct. No.]).—Tendon tissue may be well studied in the gastrocnemius of the frog. It is prepared like the preceding. If, however, it is desirable to show the nuclei in adult tissue, it is well to use nitrate of silver. Cut a thin section of a fresh tendon and expose it for a few minutes in a one-half per cent. solution of nitrate of silver, until the section is turbid or milky, then place in the sunlight, and in a few minutes the turbid colour will give place to dark brown or black, owing to the deposit of silver, and the tissue may then be mounted in glycerine and examined. This method will show the corpuscular bodies to advantage. In some

cases better results are obtained by the use of chloride of gold. The method is as follows: Freeze a thin portion of a tendon, then make the thinnest possible section, acidulate it slightly, and then immerse in a one-half per cent. solution of chloride of gold, until a straw-yellow colour has been obtained, and then immerse in a one-fourth per cent. solution of dilute acetic acid, and expose to the sunlight until it is purple or reddish; this will take a variable time, and is not always successful, for reasons which are not easy to understand. This is the ordinary method now in general use. At considerable distances from one another there will be seen small dark bodies, which are the corpuscles already described. It is difficult to show that these corpuscles are connected together. To isolate them, take a small piece of young tendon tissue, immerse three or four days in a 10 per cent. solution of common salt, and then tease. In this way the cells may be liberated, and they will be found to be irregularly flattened plates. Silver or gold, the latter especially, is generally necessary to show the nuclei in old tendons. The same method shows the fibrillated tissue to advantage. These latter methods will also show that the tendon bundles are covered, more or less completely, with a delicate epithelium (endothelium). The tendon corpuscles do not by any means form a connected sheath for the bundles. In very young tendons the corpuscles are very near together, though even then they only form a partial investment for the bundles; but as the tendon grows older the corpuscles become smaller, withdraw from one another, and sometimes almost disappear.

6. *Fat-Tissue.*—This is a form of tissue that seems to be the ordinary fibrillated connective tissue in a changed condition. It becomes the deposit for oil which appears to fill the corpuscles, making them swell out enormously, as already stated. An excellent way of showing this tissue consists in making a section of a portion of fatty tissue that has been hardened in alcohol or Müller's fluid, or both. The appearances will, in this way, be well shown. After immersion in an acid solution, it will be seen that the fatty acids crystallize in the centre of the sac.

The nature of the evidence that the fat-corpuscles are really the transformed corpuscles of the fibrous tissues is as follows: They occupy the same position, being in rows between the bundles and corresponding in position to the other corpuscles that we have mentioned; a few oil-drops at first appear, then others, until finally they coalesce into a single large drop which fills the envelope; if fat-tissue be pressed and the oil escapes, the walls of the fat-corpuscles collapse, and then the flattened plates (nuclei) may be observed on the side of the cavity.

Waldeyer (*loc. cit.*) believes that there is a peculiar corpuscle, three to five times the size of a lymphoid corpuscle and roundish,

which is especially prone to take up fat and be converted into a fat-corpuscle.

These corpuscles have recently been noticed in most of the connective substances, but it seems uncertain whether they are undergoing fatty change as a physiological or pathological act. They are said to occur constantly in the skin disease known as zanthelasma, and I have found them frequently in diphtheritic membranes, where they appeared to represent the corpuscles of the imperfectly developed tissue of the membrane, on the third or fourth day of the disease.

7. *Intermuscular Tissue.*—It has been claimed by some that there is a form of spindle cell in the intermuscular tissue in the thigh of the frog. This, however, is apparent rather than real. We find broad plates which are oval flattened bodies placed at certain distances apart (Fig. 8, Plate CLIX.). These seen in profile appear spindle-shaped. There is something peculiar about these bodies, for they seem to bear a close relationship to the elastic networks (*a*), so that in some cases it appears as if the flattened central bodies were directly connected with the elastic fibres as stated by Boll.* In many instances these elastic fibres lie upon the plates (*b*). The broad plates rest in a homogeneous, intermediate, and apparently structureless substance. In this tissue, therefore, it has not as yet appeared that there are fibrils in the intercellular substance. On the contrary, this substance is soft and homogeneous and highly elastic, and gives the appearance of a tissue that has retained to a great extent its embryonic form.

Connective Tissue of the Kidney (Fig. 9, Plate CLX.).—Here also the plate-like corpuscles may be seen (*a*) as distinguished from the lymphoid corpuscles (*b*), though the exact nature of the intercellular substance, whether fibrillary or not, is difficult to determine with satisfaction. A normal kidney thoroughly injected through both vein and artery was employed in order to differentiate completely the vessels, with the corpuscles in their coats, from the sustentacular tissue or supporting substance proper.

8. *Corneal Tissue.*—There has been a great deal of discussion within the past few years as to the structure of the cornea, and the views of observers have differed according as they have confined their attention to the interlamellar spaces or to the bodies in them. The term *corneal corpuscles*, strange as it may appear, is even now used of the spaces by some of the best-known writers, and it seems evident that there is still doubt as to whether any real corpuscles exist or not. Recently, this subject has been restudied by Waldeyer, and we have been able to verify his conclusions in a very great measure, both as to the character of the corpuscles and the spaces in which they lie. These bodies appear, as stated by Wal-

* 'Arch. für mikrosk. Anat.' 1870.

deyer, to be in general flat, having a considerable amount of protoplasmic material about their nuclei (Fig. 10), though in the direction of the periphery they gradually taper off into thin expansions which are nearly homogeneous, and extending from them are distinct processes which in part unite with those of other corpuscles and in part end blindly. In structure these corpuscles are not materially different from those of tendon tissue and the other varieties already mentioned. In them is the same flattened oval body, which seen on the side is rod-shaped (*b*), and is surrounded by an irregular envelope that assumes almost any shape. Thus the corpuscles are not always flat, though they are usually so. Their shape depends upon many different causes, such as the method of preparing the tissue, the amount of laceration to which it is subjected, &c. The best method of examining the cornea consists in preparing it by the gold method already described.

After the tissue has been properly stained, which is known when it has taken a mauve or violet tint, the specimen may be allowed to stay in the sun; then thin lamellæ are to be torn off with the forceps, and mounted in dammar varnish or Canada balsam, after the tissue has been made thoroughly transparent by soaking in oil of cloves. It will then be seen that there are bodies within certain well-defined areas—the corneal spaces as they are called by Recklinghausen and others. These bodies are disposed at pretty regular intervals throughout the cornea and are generally flat, with rounded contours, though often they have processes extending from them in various directions. In the accompanying drawing the spaces may be distinctly seen as well as the variously shaped corneal corpuscles; one (*c*) is crowded into the prolongation of a corneal space, while another (*b*) is connected by its processes with a neighbouring corpuscle. One corneal space (*a*) is entirely empty. These differing conditions are in a measure due, probably, to the laceration of the tissue in preparing it, some of the bodies having been torn out and others forced to the side of the corneal space. There seems to be a pretty general agreement that the intercellular substance may be separated into independent fibrils, but upon this point I have seen no decisive proof. Dr. Thin has called attention to certain peculiar corpuscles which he has observed in the cornea, and which were different from the corpuscles already mentioned and the lymphoid corpuscles also met with there.* Mr. Priestley, in a recent number of the 'Journal of Anatomy and Physiology' (October, 1875), has stated his experiences in looking for these corpuscles, but decides that they were probably epithelium from the anterior layer of the cornea. Dr. Thin seems to have suspected this at one time, but he tells us that he satisfied himself that he had not committed this error.

* 'Lancet,' Feb. 14, 1873.

9. *Elastic Tissue* (Fig. 8).—We have thus far omitted the discussion of elastic tissue for the reason that it is different from the other tissues already described, both microscopically and chemically, though often combined with them. It is also convenient to class it by itself for other reasons, chief of which are that its corpuscular elements have not yet been definitely shown in adult tissue. Virchow, some years ago, stated that this tissue, as well as other connective substances, was composed of networks, the substance of the fibres containing certain markings, and he inferred that these might be the corpuscles of the tissue. Elastic fibres were, however, according to him and others, nothing but the ordinary fibrous tissue condensed. Each fibre was hollow and capable of conveying the nutritive juices.

Henle in his earlier writings regarded the elastic fibres as originating from the nuclei, of which in fact they were prolongations. Subsequently, he seems to have believed that the fibres originated in the basis substance (*op. cit.*).

Reichert could not trace the connection between the nuclei and the elastic fibres, and when the latter had formed the former had disappeared.

Boll, however (*op. cit.*), distinctly states that the elastic fibres, each one constituting an "elastic cord," arise from the plate-like cells.

Ranvier (*op. cit.*) examined tendon tissue, as mentioned before, but he was only able to find the elastic fibres after boiling the tissue for from eight to ten hours. It is proper to remark, however, here that elastic fibres are very uncommon in tendon tissue; at least they have not often been observed.

The fibres of the elastic substance are pretty readily recognized by the fact that they are not coloured by carmine or hæmatoxylin, and do not swell with acetic acid; they branch dichotomously, these branches forming, with similar branches of other elastic fibres, networks; this is the general form of adult elastic tissue, and it is probable that exceptions to this rule are, at the most, extremely rare; this form prevails in the ligamentum nuchæ of the ox, in the elastic coat of large arteries and veins, in the serous membranes generally, and in the subcutaneous connective tissue of the skin, as well as in the delicate intermuscular tissue already described. It will generally be found that where this tissue occurs in bundles it is not because there are no meshes, but rather because the meshes are compressed laterally, so that they are not apparent unless most carefully teased apart. When such fibres are broken off, their extremities curl up; further, the fibres are unaffected by boiling solutions of strong acids and alkalies, such as 35 per cent. solutions of caustic potash or nitric acid (standard preparations in common use in laboratories), unless the action is prolonged for a consider-

able time. These networks are beautifully shown by taking the mesentery of the frog in contraction and immersing it in acetic acid. The fibrillated connective tissue will swell up and become invisible while the elastic fibres will be unaffected. The ligamentum nuchæ also affords an excellent opportunity for studying this tissue by itself. To render this study more easy the tissue may be allowed to remain a few days in a 10 per cent. watery solution of common salt, and it may then be more easily teased. In the subcutaneous connective tissue of the skin the elastic fibres are well shown by hæmatoxylin preparations. Being unaffected by this staining solution they appear as bright, silk-like cords, which lie in close apposition with the wavy bundles, and the branches arch over the bundles to anastomose with corresponding branches of other bundles, so that in this way meshes are formed. Some writers have spoken of little knobs at the nodal points of the meshes, but these appearances appear to have been illusory.

Recklinghausen seems to have believed,* with Virchow, that the elastic fibres contain peculiar nuclei of their own, which in adult tissue become extremely small, and are represented by the dark markings seen in such tissues. Thin claims (*loc. cit.*) that they originate in branching corpuscles which by their coalescence form the network, and the remains of the nucleus may be shown by hæmatoxylin. These markings may, it is true, be seen in the ligamentum nuchæ of the ox, but it is doubtful whether they are nuclei or mere clefts in the tissue. Examination with such high powers as Gundlach's No. 15 immersion and Wales's $\frac{1}{10}$th fails to clear up this point. We may now review these substances as a whole, and decide as to the characters they have in common. Elastic tissue, having a wholly different significance from the others, will be treated separately.

1. The most constant form that is met with in all these tissues is a somewhat flattened ovate or rhomboidal body that assumes the colouring matter deeply. It is found in each tissue we have enumerated. In some, as in the fibrillated connective tissue, it is often larger and flatter than in others, as the tendon tissue, but this difference appears to depend upon certain conditions which modify the original form. For example, when the tissue is young or inflamed, these corpuscles are larger, more vesicular, and closer together, and have better defined edges; when, on the other hand, the tissue is older or in a state of comparative inactivity, as in ordinary health, these bodies are shrunken and irregular, and conform more to the precise locality in which they are placed; they are also farther apart, and often are hard to discover at all—facts which serve to explain the statements of Waldeyer, who says that these corpuscles are often paddle-wheel shaped, and instead of con-

* 'Cellular Pathology,' 1871.

sisting of *single* plates, as Ranvier says, they are made up of *many* plates, the paddles, which radiate from a centre. In examining these tissues with a lens of high power, such as the No. 8 of Hartnack's system, there will be no difficulty in detecting them in every instance, and there can be no doubt that the use of hæmatoxylin in these examinations has afforded us the best means of demonstrating many points which have previously been obscure. The particular reason of the advantage obtained by the use of hæmatoxylin lies in the fact, that very often it obviates the use of alcohol acetic acid, both of which distort the parts, and very often give rise to false appearances. It also chiefly affects the plate-like bodies and only slightly tinges the delicate envelope, while it wholly avoids the rest of the intercellular substance. In this way we are able to employ a solution that differentiates the elements most excellently, in fact far better than carmine, and it is dissolved in water and not in acids or alkalies, which have peculiar actions upon all tissues. During the latter portion of this work use was often made of Hoffmann's violet, which was used as a substitute for the *violet de Paris* or methyaniline (Poirier) recommended by Cornil for showing waxy degeneration. It was dissolved in water in about the proportion of two grains to the ounce. In half an hour the nuclei were beautifully stained of a delicate violet, when the cell body and fibres were unaffected. The reaction was much the same as hæmatoxylin in regard to the tissue affected.

It is with the use of the two reagents that we may get the best ideas of the structure of connective substances.

2. Most of these plate-like cells which have often been called nuclei are invested by a delicate envelope, the body of the cell. This is mentioned by almost all recent observers, and was even spoken of by Henle long ago. This substance is seen to advantage in the reticular form of tissue (fibrous tissue) already mentioned, and is not so deeply stained by hæmatoxylin. When the tissue is expanded, this sheath or envelope is capable of being drawn out to great length, while, when it is separated from its connections, it shrinks, assuming the most irregular forms. When the plate-like bodies with their envelopes are attached together, they give the appearance of "spindle-shaped cells," a name often given to them. It may readily be imagined that the changes in form which such delicate envelopes assume may be manifold. In moist tissues they may swell, and in dry ones they may shrivel. It is the processes of such envelopes that often communicate, and the term "netted cells" has often been applied to them.

The theory developed by Heitzmann* that the connective tissue of the umbilical cord, periosteum, and tendons, &c., demonstrates a

* "Uber das Verhältniss zwischen Protoplasma und Grundsubstanz im Thierkörper," 'Sitz. d. Wiener Akad. v. und h. Jahresbericht,' 1873.

continuous connection between the processes of the corpuscles, as he observed in cartilage, and is commonly seen in bone, could only be substantiated in a few instances. We have noted that, both in the cornea and in the mucous tissue of the umbilical cord and reticular tissue, there is such a connection to be sometimes seen, but at other times it is not seen at all, and this is the more difficult to understand, if the theory of a constant connection between these processes is tenable, for the method of preparing the cornea (tearing off the lamella) appears to give us a view of the corpuscles, or some of them at least in their proper connections, and the gold method defines them clearly. As we have seen, they are, however, only occasionally united. In the older forms of tissue, as in tendons, there was no such connection noted; indeed, in most of the tissues enumerated, the corpuscles with their delicate sheaths appear to be quite separate from one another.

In fat-tissue it seems, as we have already stated, that the delicate envelope takes up the oil, at first in minute globules, which by their union form larger ones and so finally completely fill up the sac. The flattened corpuscle, or "nucleus," that belongs to the tissue is unchanged, however, but takes its position in the side of the sac.

The second form of corpuscle that is frequently met with in all situations in the tissue is the round corpuscle already mentioned, and known as the lymphoid cell or corpuscle. It has often a pale, fleecy investment about it, which does not colour with hæmatoxylin, or Hoffmann's violet, or only slightly. Very similar bodies are often seen in close connection with the plate-like corpuscles, from which it often appears as if they originated from the latter. The third corpuscle similar to the one mentioned by Waldeyer and by Klein is also sometimes seen. It is large, about four or five times the size of a lymphoid corpuscle, and pretty globular in shape, and contains coarsely refracting bodies. These appear to be minute oil-drops, but whether these are the result of physiological or pathological change is uncertain. The adult intercellular tissue is made up of bundles of indefinite length. As for the bundles, each one is made up of separate fibrils which do not anastomose but run a parallel course. The fibrillar connective tissue, adenoid tissue, neuroglia and tendon tissue have this character clearly, while in the other forms it is not so certain that this is the case. The fibrils are held together by a firm cementing substance which can be dissolved by long immersion in Müller's fluid, or for a few days in a 10 per cent. watery solution of common salt.

3. We can now study the relations of these parts to one another. It may be stated as a fact that is indisputable, that in all the adult tissues, excepting, perhaps, the supporting tissue of the kidneys and, of course, elastic tissue, and of the cornea, there are two principal substances met with, more or less flattened cor-

puscles and an intermediate fibrillar substance. The exact relation of the fibres to the cells has never been thoroughly stated of all these tissues, but it seems proper to conclude from the foregoing statements that in each instance the plate-like cells are superimposed on the fibres and form in this way, often, a partially investing sheath. The envelope which is the investing substance of the plate-like cells forms a bed upon the bundles to which they adhere very strongly. By their processes these envelopes anastomose with other adjacent bodies of like structures; they do not always anastomose, however, or do not appear to, and may be quite free. In the cornea they often unite and often do not, but, in so doing, they extend their processes through the channels which are supposed to convey lymph from the corneal spaces. Klein's view that netted cells form the network in adenoid tissue is untrue in many cases, judging from the observations that were made, for it was found possible to brush off many of the bodies, showing that they were on the tissue and not in it. These so-called cells were also seen by sufficiently high powers to be fibrillated, and therefore not "netted cells," but bundles of larger or smaller size.

Elastic Tissue.—1. There are no corpuscles that have been found in adult elastic tissue that can be positively shown to belong to it exclusively. In young tissue they certainly occur, according to excellent authorities, but they do not maintain their integrity long. Possibly higher powers than those in use may discover the bodies mentioned by Recklinghausen and Thin, but at present they elude our observation.

2. The fibres are cylindrical and branch dichotomously, and they exhibit an indifference to micro-chemical reagents that is not shared by others of the connective-substance group. They do not appear to be made up of fibrillæ, but each elastic cord is the ultimate element. They have no necessary connection with the other connective substances, and that they are not always present may be shown by boiling adenoid tissue and tendon tissue in the solutions of caustic potash before mentioned, when it will be found that these substances will dissolve entirely, which would not be the case if elastic fibres were present, for they resist the action of boiling acids and alkalies, as we have seen. We have stated that in the intermuscular tissue of the frog's thigh, which is extremely rich in elastic networks, there are numerous large and flattened corpuscles, which rest, apparently, upon broad and flattened plates of a fibrillated character, and here it often appeared as if the flattened bodies were continuous with the firm branches of the network. In other cases the elastic fibres pass directly over the plate-like body. Whether, however, there is a connection between them as Adickes* claims, is difficult to determine.

* 'Archiv. der Heilkunde,' iv., 1872.

The Development of Connective Substances.—Opportunities for studying this portion of the subject were given by the examination of different portions of the *umbilical cord* of an embryo of three months, also *cicatricial tissue* which had been removed from the face on the third day, from the *fibrous alveoli* of a cancer of the breast, and a *fibrous thickening of the scalp*, which developed rapidly from a bony tumour of the calvarium on which it lay (Fig. 11, Plate CLIX.). In the cicatrix removed on the third day, plate-like cells were found measuring from $\frac{1}{1200}$ to $\frac{1}{2000}$ inch in diameter, and surrounded by a nearly hyaline membrane.

The central body was about the size of a lymph corpuscle, and whenever fibres occurred they were arranged in parallel rows, and upon them were the flattened corpuscles surrounded by a hyaline substance. In one instance the central body was dividing. In examining the fibroma of the scalp, the bodies were seen to be plates (*b*), though ammonia-carmine did not show them, and it was not until acetic acid was used that they became granular and in this way apparent. The intercellular substance was seen to consist of flattened ribbon-like laminæ tapering off at each end. It seemed as if fibres ran over these corpuscles and were continuous with the elastic fibres, but this appearance was by no means constant, and when gold was used these plates appeared, at the end of twelve hours, to be irregular rhomboidal (Fig. 11, *a*). In the alveoli of a growing carcinoma, the plate-like cells were also seen, but the intercellular substance did not give the uniform appearance of fibrillation, owing perhaps to its early age.

These specimens also afforded instructive testimony of the fact that there is apt to be great confusion in the use of the word "cell," for it may often be shown that the so-called spindle cells—by which are here meant some of the larger figures embraced under that name—are often not such at all. A good lens shows that the so-called spindle cell may be a thin ribbon-like portion of the intercellular substance that we have learned to know, upon which is the flattened corpuscle, itself surrounded by a more or less delicate investment, generally of a hyaline material. The gold method brings out these points in great perfection. Further proof in support of this statement may be gained by pressing such a spindle cell between the cover and the slide in the way already mentioned. The "nucleus," or flattened corpuscle, may then be made to drop off. The valuable results that follow from recognizing these points are, that they at once give us an insight into the structure of the so-called spindle-celled sarcomata or fibro-recurrent tumours which are formed of a tissue very similar to some of the connective substances already mentioned, for in describing them the very same error has often been committed as in calling the plate-like cells spindle-shaped. Anyone who has examined

such a tumour in the way just described, will have no great difficulty in demonstrating to himself that the so-called nuclei of the spindle cells are, in reality, very similar both in form and size to the plate-like corpuscles already described in the umbilical cord. These facts were elicited from the study of such a sarcoma, whose character was definitely established by its microscopical character and its frequent recurrence (five times). The younger portions were known to be such by the description given of them by the patient, for, the tumours being nodular, the age of each nodule was pretty nearly established. In the young portions, the same plate-like bodies as are found in the umbilical cord, or bodies at least in general very similar, were found, but they were imbedded at intervals in a homogeneous material in which as yet there was no fibrillation. This portion was actually a young growth. Older portions were then examined, i. e. those known clinically to be older. It was found that the intercellular substance had a fibrillated appearance, and by suitable reagents it could be broken up into thin ribbons or bands, as by Müller's fluid or by a 10 per cent. solution of common salt; numerous long spindle-shaped figures, having a flattened body at their centre, were then found; and there were also similar spindle cells without any central body or "nucleus." Where, however, such appearances are observed it is easy to introduce a current, roll these bodies over, and then it may be seen that they are long, flattened, and of irregular size, appearing on profile view to be spindle-shaped, and yet we may often press off the "nuclei" by pressing the cover upon the slide, showing conclusively that such spindle cells are really the intercellular substance at an early stage of fibrillation, and almost precisely what may be seen in certain parts of the umbilical cord of young embryos as already described. It does not, however, follow from this that all of the spindle-celled sarcomata are of these varieties, indeed we sometimes see them where they appear to be composed of real spindle-shaped bodies, closely packed together, and where each body contains within it a smaller flattened body. From a study of these gradual changes it seems likely that, in growth and repair, the corpuscles at first *round* soon become *flattened*, and have a broad envelope (*b*). About this envelope there is a further delicate and lightly attached investment, which, uniting with the investments of other similar bodies, is the commencement of the intercellular substance. At first the plate-like bodies lie in niches, as it were, in the intercellular substances, and if one is brushed out it leaves a socket behind it (*c*). As the intercellular substance increases, the corpuscles are arranged in rows and they become smaller, while immediately under them thin laminæ are formed from the effused fibrin—the commencement of fibrillation—while the corpuscles are unchanged except that they become

smaller, their envelope shrinking, and they recede from one another. In advanced life these corpuscles are generally more or less flattened, but their form is also considerably modified by the age of the tissues, and various mechanical alterations to which they are subjected, according to the particular locality in which they occur. —*Read before the Biological Section of the New York Academy of Sciences,* May 1, 1876.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Sponges of the Channel. Their Development.—M. Charles Barrois has just published his valuable Thesis for the French degree of Dr. es Sciences Naturelles. It is dated July 1876. The following are the conclusions which the author has drawn. The author's observations were conducted with the view of making clearer the development of those sponges which belong to the most distinct group of the entire class. What the author has seen in the development of the sponges has led him to the conclusion that all the different groups present the same essential processes of development, but that the stages of development appear in a different order and more or less modified by various circumstances in the several groups. The general mode of development that he believes he is able to set forth from his observations seems to be *not* a *Gastrula fixée en Hydraire*, and the *endoderm* of which ramifies in the gastro-intestinal system; but a compact mass composed of two superimposed leaflets, the external of which represents the exoderm, the internal stands for the union of the internal and median leaflets. This is, according to the author, the common form in the different species of sponges. The egg of the sponge appears in the formative layer of the skeleton —the *mesoderm* of F. E. Schultze. It presents at first the same characters in the whole group; but soon the formation of pigment and of pseudo-cells distinguishes the silicious sponges from the others. The author has never been able to see the process of fecundation. The segmentation is entire and regular; each group presents peculiarities in the progress of this phenomenon, but nevertheless the result of it is constant; it produces a cavity of segmentation and finally a generally hollow sphere. This sphere differentiates itself into two distinct parts in all sponges; the elements which will form the exoderm appear at one pole, and those which will form the other leaflets appear at the opposite pole. These processes present interesting peculiarities; thus, whilst the distinction is recognizable in the calcareous and fibrous sponges in the early periods of segmentation, it is not apparent in *Halisarca* and *Halichondrida* until the embryo arrives at the state of free larva. When the sphere is thus differentiated into two distinct histological halves, it produces in the *Calcispongiæ* an invagination of one of these halves into that which represents the exoderm; this is but a transition stage that he has not seen in the other families of sponges. Then follows *devagination* of the *Gastrula* of calcareous sponges; the limit between the two halves of the sphere thus produced corresponds to the former so-called mouth of the *Gastrula*. This part is clearly distinct in the free larvæ of various families of sponges; it is represented by a regular crown of cells in the calcareous sponges, by a crown of large flagella in the fibrous and silicious sponges, but it is less distinct in the larvæ of *Halisarca*. This crown is the starting point in the formation of the spicules; it is the sole representative of the mesoderm of the

larva. It has its greatest development in those sponges whose mesodermic products are most abundant (in the spiculated sponges, for example); it is reduced in the sponges without spicules (for instance, *Halisarca*). The formation of spicules furnishes a new example of Heterochrony (*hétérochronie*). They are formed in the *Halichondrida* before the animal has become fixed; they do not appear till after fixation in the *Calcispongiæ;* at least it is so in the normal condition. The generality of the appearance of spicules with a ray before that of spicules with many rays has a certain importance in the history of genera. The cellules of the larva which form the exoderm of the sponge are long, transparent, and ciliated; they form in the various groups the anterior part of the embryo; the elements which represent the two other leaflets differ more generally according to the species. Thus in the *Calcispongiæ* they are composed of large and rounded cells, in the *Myxospongiæ* the cells are prismatic with short flagella; in the *Halichondrida* they are united in a continuous *plasmodium.* In these last sponges these leaflets are produced by a delamination of the internal part of the larva; in the other groups they are produced by a direct differentiation of the posterior part of the larva. But in both cases the result is the same, thanks to the extension outwards towards the posterior part of the larva, of the mass of the internal laminæ or leaflets (*feuillets*). The fixation of the larvæ takes place by their posterior part, i. e. by the laminæ which normally form this part. At this period the young sponge is, in the different groups, a compact mass composed of two superimposed laminæ, the outer one representing the exoderm, the inner one representing the reunion of the *internal* and middle laminæ; the different groups are only distinguished by their spicules. The young fixed sponge differs from the larva only by its flattened and irregular form. The first phenomenon presented by this young sponge is the separation of the inferior lamina into endoderm and mesoderm. This phenomenon manifests itself by the appearance of special endodermic elements circumscribing a peculiar system of cavities. This is the *system of endodermic cavities*, the most important of these systems in the classification point of view. It is represented by the vibratile bunches of *Leucon* and *Halichondrida*, and by the radiating vibratile tubes of *Sycon.* There are then many other systems of cavities in young sponges. One of them, which the author terms a "*system of mesodermic cavities*," is a system of canals within canals. It is produced by the formation of irregular vacuoles which extend into the mesoderm between the vibratile organs. A third system of cavities is that in which the sponge takes part in its entirety. Of this we see examples in the *Sycon*, *Poterion*, *Veluspa*, and other silicious sponges of the cup form. A fourth system of cavities is that which is formed by the incomplete fusion of various portions of the polypite itself. The importance of characters being regulated by the order of their appearance in the embryo, the most important character for the natural classification of adult sponges is considered by M. Barrois to be the spicules. The disposition of the first system of cavities comes next in order. Then follow afterwards the appear-

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ance of the openings, the arrangement of spicules, and the formation of fibres. M. Barrois limits the term of *oscules* to the apertures of the cavities of the mesodermic system. They are homotypes of the pores.

Histology of the Hœir.—This subject has been recently worked at by Herr Ebner, who has presented a memoir upon it to the Viennese Academy of Sciences (July 12). Among other things he shows that the inner root sheath is essential for hair formation, and though broken through by the hair, it grows during the whole hair vegetation, in the lower part of the follicle with even greater rapidity than the hair. He defends Langer's view that the new hairs are formed in the old follicle and on the old papilla, and describes fully the mechanism of the progress.

The Anatomy and Development of the Brain in Fish-like Vertebrates has been the subject of a recent communication to the Philadelphia Academy, by Professor B. G. Wilder, of Cornel University. It is published as a brief Report in the 'American Naturalist' for August, which states that after considering the taxonomic value of the brain, he spoke of the investigations of Huxley, Owen, and the continental naturalists, dwelling particularly upon the causes of the great inaccuracy in the figures of fishes' brains contained in the text-books. He had endeavoured to ascertain how far the brains of fishes might be homologized with the typical brain described and figured in diagram by Huxley. The differentiation of the three typical cerebral vesicles was described, and the fact stated that while the typical description applies to all the higher vertebrate brains, neither the lateral ventricles nor the foramen of Munroe had been observed in the brains of fishes until recently found by Professor Wilder in the gar-pike. He had since found them in the lamprey and the hag-fish, in several sharks and skates, in sturgeons, in the spoon-bill sturgeon, in the mud-fish or Amia, and in several typical bony fishes. He showed in what way the nearly solid front mass of the adult shark's brain is formed from a thin-walled vesicle in the embryo. The structure of the brain in ganoids and teleosts was described, and the distinction indicated that in the latter, although the lateral ventricles and the foramen of Munroe are present, they are so small as to be almost invisible. We are forced back, therefore, in searching for the distinctive character of the ganoid brain, upon the chiasma of the optic nerves of Müller. In considering the taxonomic value of these characters, the belief was expressed that the structure of brains will be found to be less dependent upon external modifying circumstances than are other parts of the animal organization. In conclusion Professor Wilder exhibited and described the brain of Chimæra, and indicated its relations to the other groups spoken of. He regarded the brain as presenting characters intermediate between the sharks and skates, the ganoids and the batrachians with Lepidosteus.

Rotifers within Volvox.—This fact, which has been noticed over and over again, has a recent note upon it in 'Science Gossip' for October. The writer, Mr. G. F. Chantrell, Hon. Secretary of the

Liverpool Microscopical Society, states that in "answer to the inquiry in a recent number (August), as to the Volvox only being met with in June, in my experience I have met with them this year in May, and every month up to the present, and last year I certainly met with them in September. With respect to the Rotifers in Volvox, they are mostly found in the latter months. I have seen specimens almost daily the last three weeks, and not only Rotifers, but Rotifers in the egg, showing movement of the cilia and of the gizzard, and also eggs not so far developed. In *one* Volvox which I examined last night I saw three active Rotifers, two eggs showing movement, four eggs similar but quiescent, and four green spheres; *thirteen* in all. There is, in my opinion, no question that some Rotifers are developed *in* the Volvox. The study of the Volvox at this particular time is well worthy of the attention of the microscopist."

The Development of the House-fly.—This, which was very thoroughly worked out in the Boston Societies' Reports a couple of years ago, has had an interesting paper devoted to it in the August number of the 'American Naturalist,' by the same writer, Mr. A. S. Packard, jun. After describing the different methods adopted by him to secure an abundance of the ova, the writer goes on to describe a mode of exposing manure, which seems to have been very successful. Then he says: "The egg of the house-fly is long, slender, cylindrical, and a little smaller at the anterior end than at the other. It is ·04–·05 of an inch long, and about one-quarter as thick. The shell is so dense that the early embryonic phases could not be watched, but enough was seen to enable us to determine that the mode of growth in the egg is nearly the same as that of the flesh-fly, as observed by Dr. Weismann. The eggs thus laid were found to hatch twenty-four hours later. In confinement they required from five to ten hours more, and the maggots hatched in confinement were smaller than those reared from eggs deposited in warm manure. Certain worms reared also in too dry manure were nearly one-half smaller than those bred in more favourable circumstances. For several days the worms living in this dry manure did not grow sensibly. Too direct warmth, but more especially the want of sufficient moisture, and consequently of available semi-liquid fluid, seemed to cause them to become dwarfed. It is evident that heat and moisture are required for the normal development of the fly, as they are for nearly all insects. The maggot molts twice, consequently there are three stages of development, and it becomes sensibly larger at each stage. After remaining in the first stage for one day it molts, and differs from the preceding stage only in being a little larger, and in the addition of the spiracle near the head. After remaining in this stage from twenty-four to thirty-six hours, it sheds its skin and enters upon the third stage, which lasts three or four days. The body is long and slender, somewhat conical, the head and mouth-parts being rudimentary. The end of the body is truncated, and bears two short tubercles or spiracles. If we enlarge one of these circular breathing holes we may see three sinuous openings, the edges of which are armed with fine projections, forming a rude sieve for the exclusion of dust and dirt. With these spiracles connect the

two main tracheæ, communicating by two cross branches and sending off numerous twigs. The young of the house-fly differs chiefly from that of the flesh-fly in being only one half as large, while the form of the openings in the spiracles at the end of the body is entirely different. When about to transform into the pupa or chrysalis state the body contracts into a barrel-shaped form, turns brown and hard, forming a case (puparium), within which the body of the larva transforms into that of the pupa. Weismann has made the discovery that in the larval flesh-fly, when about to transform into the pupa state, the head and thoracic segments die, and that the head and thorax of the pupa arise from minute disks attached to the smaller nerves or tracheæ in the body of the worm. This is paralleled by the metamorphosis of the 'pluteus' into the adult starfish, and is a much more complete metamorphosis than even that of the caterpillar into the chrysalis of the butterfly. Our house-fly having as a maggot lived a life of squalor, immersed in its revolting food, with its new change of form, involving the death of one half its body and the origin of a new head and thorax, with legs and wings, eyes, feelers, and mouth-parts, after a short pupal sleep of from five to seven days pushes off one end of its pupa case, and appears winged, with legs where before there were no traces of feet, and is animated by new instincts and mental traits. It is difficult to realize how striking are the changes, physical and psychological, which the house-fly undergoes in the transition from the maggot to the volant, cursorial being."

The Fructification of the Basidiomycelis.—A recent number of the 'Academy' contains a note on the above subject which refers to a paper by Professor Reess in the 'Sitzungsberichte' of the Physico-Medical Society of Erlangen. It says of this botanist, that he describes the ripe spore of *Coprinus stercorarius* as opaque and seemingly homogeneous in a dry state. When it has absorbed water, "the inner contour of the brown episporium is plainly seen. The existence of the endospore is, however, first proved by the germination. The spore-contents exhibit nothing remarkable. The germination of the spore occurs in water or on moderately dry dung, upon which it ripens. In fresh dung, or dung decoction, it begins in a few hours and proceeds quickly; more so if the decoction is concentrated than 'if it is weak.' It commences with the extension of a round papilla of the colourless endospore from one, or less often from both ends." The author describes the formation of rods in diverging groups, of which he gives a figure. These rod-cells are spermatia. The Coprinus likewise forms corpogonia, figured as rounded swellings filled with granular protoplasms. Two other figures show the rod-cells attached to the carpogonium cells, which they fructify. The sexual organs and fructification of Coprinus, Professor Reess says, resemble those of lichens and *floridiae.*

A Curiows Fact in the Development of Bursulla crystallina.—Under the name of *B. crystallina* Professor Sorokine* describes a new genus of Myxomycetes in the 'Annales des Sciences Naturelles.'† This

* 'Academy,' Sept. 30. † Sér. 6, vol. iii. part 1.

organism was discovered on horse-dung, and is nearly related to the curious *Guttulina rosea* of Cienkowsky, differing chiefly in the presence of a common membrane in the organs of fructification. In watching the development of this organism, Professor Sorokine stumbled upon an interesting fact. Wishing to ascertain the effects of a low temperature upon it, he placed some of it in the open air during the month of December, when the thermometer ranged between 5·5° and 23° below zero Fahr. A few days afterwards he observed that some of the sporanges contained portions of protoplasm and a distinct nucleus exactly in the centre of these fragments. Thus, although there are two kinds of organs of fructification produced on the surface of the dung, it is easy to distinguish them by the presence of a nucleus in the organs of later formations. Nevertheless, the writer was unable to determine which sexual part each of these two kinds played. They are quite distinct, and the result of their union is the formation of a cellule which may be termed an oosphere.

Observations on the Protista.—'Nature' published recently a very interesting letter on this subject from a German correspondent. He states that Herr Cienkowsky, who several years ago made some exceedingly interesting communications on the low organisms known as Monads,* has recently contributed some additional information regarding them and allied organisms.† To the lowest order of plants belong the Myxomycetes, which, in the complete state, form protoplasmic nets, named plasmodia. Cienkowsky found such plasmodia in fresh water, which fed themselves by suction of algæ; on passage into the resting state, they fell asunder into several cysts, and (what is deserving of special attention), by the release of small portions from their mass, produced amœba, i. e. self-supporting individuals, which creep about by means of pseudopodia, and which have hitherto been regarded as independent animal organisms. As this phenomenon has also been observed in other plasmodia (Brefeld), it is not improbable that very many amœba do not represent independent forms, but belong to the development cycle of other and plant-like forms. *Ciliophrys infusionum*, an organism which stands very near the animals named Actinophrys, is transformed, while under the covering glass, into a swarmer (swamspore), and when several individuals are connected, or one enters on the process of division, there arise as many swarmers as there were parts. Through this formation of swarmers there appears Heliozoa, which group belongs to the Actinophrys, closely related to Monads, or those lowest organisms which have been claimed both by zoologists and botanists as objects belonging to them. Among the Monads, Cienkowsky observes various encystments, divisions, and colony formations; but the most remarkable of such processes is that in *Diplophrys stercorea*, an extremely small cell-like organism with a yellow spot, and pseudopodia at two opposite ends of the body. These little bodies, observed in moist horse-dung, multiply by division, and form by union of pseudopodia, long strings in which separate individuals can glide to and fro. In several of the organisms he examined,

* 'Archiv. für Microscopische Anatomie,' i., 1865. † Ibid., xii., 1875.

Cienkowsky was able to observe the taking up of solid food by suction of algæ. Thus the boundary lines, which it has so long been usual to draw between plant and animal organisms, and between the individual groups of those lowest forms of life, appear more and more illusory, and the supposition is recommended of a common lowest kingdom of organisms, that of Protista (Haeckel), out of which animals and plants have by degrees been differentiated.

The Functions of the Hairs on the Rootlets of Plants is thus explained by Mr. B. C. Halstead, in a late number of the 'Gardener's Monthly.' He states that the largest portion of the liquid used by the growing plant makes its entrance through the roots, from the soil, is a well-established fact; but those parts which are the most active in the absorption of this food material in solution were for a long time not so clearly understood. By careful experiments and microscopic investigation, it is found that the extreme tips of young roots are about the only portion which take little or no part in this work. A short distance back from the growing points, on nearly all growing roots, may be seen with the aid of a microscope a large number of minute, slender bodies, extending out in all directions from the surface of the root. These thread-like structures are not inaptly called root hairs, and consist of sac-like protuberances, as outgrowths from the epidermis or surface cells of the root. With the naked eye they are not easily seen; but their presence may be inferred from the manner in which they cling to the particles of the soil when a young root is lifted carefully from the earth in which it was growing. This power, which they have of fixing themselves to the grains of earth, is very great; so that, when a plant is taken violently from the soil, large portions of these delicate hairs are broken from the roots, and retain their attachment to the soil. As the root grows along in the earth new hairs are produced, while those behind perish as the root becomes woody, and a dense, non-absorbing, protecting epidermis is formed; so that the active life of a single hair is of short duration. The office of these hairs must have already suggested itself to the reader. By means of these prolongations the greater part of the absorption takes place, though the newly-formed surface cells are also active.

Microscopic Anatomy of the Oviduct of Cistudo Europœa.—The 'Archives de Physiologie' (No. 3, 1876) contains an important paper on this subject by M. F. Lataste. It records results obtained at the histological laboratory of the College of France. The following are the more valuable conclusions. He says that there are five membranes to be distinguished in passing from within outwards: (1) A mucous epithelium, vibratile at first, with laminæ of caliciform cells; this layer diminishes in thickness from above downwards. (2) A connective layer, whose thickness goes on increasing from above downwards; without glands above, then containing glands of two kinds, first mucous with caliciform cells, then others of a special kind. (3 and 4) Two muscular layers, absent from the upper part, where they are replaced by smooth fibres in the midst of connective tissue;

then showing themselves successively as an internal layer with concentric fibres, and an external with longitudinal. (5) Lastly, a flattened epithelium, the peritoneal serous epithelium. The author gives a very good plate, which illustrates these layers with distinctness.

The Mode in which the Young of the New Zealand Astacoides attach themselves to the Mother.—In a paper in the 'Annals of Natural History' (October 1876), Mr. J. Wood-Mason describes an interesting structure, by means of which the young of the New Zealand crayfish attach themselves to the mother. Indeed, it strikes us that the idea of the young being attached at all is decidedly a novel fact. Mr. Mason gives a magnified figure of one of the limbs of the young *Astacoides*, which shows us that at the extremity it is furnished with a peculiar hook-like process. This so firmly sticks in the mother, that Mr. Mason had to pull the young animal away without its limb in order to disengage it.

Deep-sea Sponges and their Spicules.—Mr. H. J. Carter, F.R.S., continues his valuable papers on this subject in the 'Annals of Natural History.' In the number for October he describes and figures many points in their microscopic anatomy. His paper is not completed, but is to be continued.

A New Hydroid Polyp Tiarella singularis.—Herr F. E. Schultze describes a new polyp, which he recently discovered in the Mediterranean, and which he calls as above. He figures it as a whole, much enlarged, and he gives a plate illustrative of its microscopic anatomy. He describes minutely the *perisarc*, *cœnosarc*, and *hydranth*, and gives full details as to its reproductive apparatus.*

Vascular Networks of the Eye in Vertebrates.—M. Beauregard has certainly exhausted this subject in the splendid memoir he has published in the 'Annales des Sciences Naturelles.' † In a paper of 158 pages, with six admirable plates, he goes into the structure of these apparatus as they present themselves in birds, reptiles, batrachians, and fishes. Nay more, for he goes into the developmental stages of each. The paper is of extreme length, and the author deals fully in the preceding part with the views of many of his predecessors in the field. The most novel part of the work is that in which he describes how he applied the ophthalmoscope even to the eyes of fishes, and the means by which he kept up artificial respiration whilst he was observing the structures. In some cases this lasted for hours.

The Development of the Crustacean Embryo.—The 'Proceedings of the Royal Society' (No. 168) contains an abstract of a capital paper by our very best English authority on this subject. Mr. C. Spence Bate, F.R.S., who is its author, states that although the general forms of several genera of Podophthalmous Crustacea are known, yet the details of their structure have been so unsatisfactorily figured and described, that the value and importance of hereditary elements are incapable of being studied and appreciated.

* *Vide* Siebold und Kölliker's 'Zietschrift,' 27th Band, 3 Heft, 1876.

† Tom. iv., No. 1 to 3.

Through Dr. Carpenter he received from Mr. Power an offer of a considerable number of larvæ of exotic species, together with the parents from which they had been obtained; in relation to which Mr. Power wrote:

"Dear Sir,—I have to thank you for your kindness in answering my letter to Dr. Carpenter, and for the memoirs.

"My collection of Crustacea and the microscope slides of the larvæ are at present, and have been, packed up in Fort Louis. Now I am again on detachment; and if left here in peace for a few months, I shall arrange my specimens and finish up the microscopic drawings.

"All my larvæ are hatched in basins (the only kind of aquaria my nomad life allows me to use), so each crab or prawn, &c., whose larvæ I possess is identified with its young; and this reminds me that on reading Fritz Müller's paper in the 'Annals'* I was much astonished, as none of the prawns or prawn-allies whose young I have hatched show any such *Nauplius* form as shown in figures 1 and 3, &c.; but all I have observed as yet are born like fig. 8, or near it.

"I have been quite unable to rear any crab-larvæ beyond a day or two after birth; whether they require moving water or not I do not know; but certainly, though I have kept the parents alive for several weeks in basins (the water changed once or twice in twenty-four hours) of salt water, the same method would not succeed with the larvæ. I then tried small aquaria, and signally failed again.

"I have not been in the neighbourhood of fresh water as yet, so have had no opportunities of observing the fresh-water Crustacea, though there are a good many crab and shrimp forms. I have found two kinds of that curious parasitic crustacean which adheres like a little polypus, a mere bag with a peduncle, but containing hundreds of young Crustacea whose genus I do not know, as I cannot find any account of them in Van der Hoeven's 'Zoology.'†

"If I succeed in getting posted to one of the regiments here, my life will be more stationary, and I shall have far better chances of working my crab-hatchings.

"In Fritz Müller's paper before referred to, I fancy that he has not hatched the different larvæ mentioned. After reading the paper very carefully, I could not help fancying that the various stages of development were not hatched through, but specimens were captured at different times, and perhaps larvæ of totally different species have been given as stages of the same animal. I say this with great doubt; but reading the paper will, I think, bring everyone to the same conclusion. Thus he says, 'the unaltered *Nauplius* form, probably the same in which the animal escapes from the egg, came under notice only once;' again, 'This larva (taken on the 13th of January) is closely approached by four others, probably *belonging* to the same swarm, which were taken at the same time (24th January);' and so on.

"To tow a net in these tropical seas and to examine all the micro-

* 1864, vol. xiv. p. 104.

† [New genus allied to *Sacculina*, which hatch larvæ in the cirriped pupa stage.—C. S. B.]

scopic Crustacea would give a most extraordinary assemblage of forms; but I doubt if it is so useful as tracing the steps of individuals.

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"I have not yet hatched the land hermit-crabs, though I suppose they are much as the ordinary sea specimens, and they certainly spend their larval life in the sea.

"Yours very truly,

"WILMOT HENRY POWER,

"*Staff-Surgeon, 44th Regt., Lt. Inf.*"

Some time afterwards the author received the promised collection, together with Mr. Power's drawings and notes. These have enabled him to identify the parent forms of some known larvæ, and also to determine those of several unknown genera.

It has also led him to the conviction of a unity of character throughout the various forms and changes of Crustacea; that variety in form is never inconsistent with homological truth; that parts suppressed or rendered abortive for want of use are never absolutely lost, and may be reproduced under conditions that may require them.

The eyes of those Crustacea, such as *Alphœus*, that inhabit dark places are reduced in power according to the condition of their habitat. But these organs are, in their larval state, as well developed, if not more so, as any of those whose life is passed in the bright sunshine of the surface of the ocean.

The blind *Didamia* brought from the depth of four miles below the surface of the Atlantic by the dredges of the 'Challenger' differs in no respect from *Polycheles*, taken by Heller in the comparatively shallow Adriatic sea. In the blind prawn from the Mammoth Cave of America, and the sightless *Nephrops* of Formosa, the organs of vision are reduced to the smallest condition consistent with their retention; and in the Cirripedes the eyes are represented by their nervous apparatus only.

The several forms of larva have not, in the prawn-allies, shown any approach to the *Nauplius* state, as mentioned by Fritz Müller, so that the author believes that it must be confined to the genus *Penœus* alone among the Podophthalmia. Nor should it be forgotten that the *Nauplius* form has only been observed as a free-swimming animal.

The author has taken this opportunity of making a close examination into the earlier stages in the development of the embryo, and comparing the progress within the ovum of some of the larvæ that arrive at or near maturity before being hatched, with those of the larval forms that are hatched in a more immature condition; and he states that, as soon as the protoplasm assumes anything like a definite plan, distinct lobes, corresponding in position with those of the several appendages in the *Nauplius*, together with an embryonic or ocular spot, are present; that in the *Nauplius* forms they exist as deciduous appendages only, and are soon cast aside and replaced by others more adapted to the wants of the adult existence.

In the embryos of other Crustacea the anterior pair of lobes enlarge in size with little alteration of form, while the posterior two

pairs are developed into appendages that have but a deciduous value, since they never fulfil the office of permanent organs, and are generally cast off with an early moult.

This is observable within the ovum in *Palæmon*, *Crangon*, &c., and also in the marsupial embryo of *Mysis* after it has quitted the ovum.

The relation of these parts to the permanent organs the author has closely traced, and believes that he has demonstrated that the three pairs of mobile appendages in the cirripedal or *Nauplius* form of larva homologize with the eyes and two pairs of antennæ, and not with the antennæ and mandibles, as stated by Fritz Müller, Anton Dohrn, and others.

The author, moreover, contends that the small pair of filamentary appendages seen on each side of the ocular spot, existing in the *Nauplii* of Cirripedes, homologize with the peduncular appendage existing in the larva of *Caligus*, the arm-like appendages in the pupa stage of Cirripedes, the peduncle of the stalked Cirripedes, and probably also with the long multiarticulate, antenna-like organs belonging to the fossil *Pterygotus*.

He also demonstrates the origin of the nerves in a mass of cellular material that reaches from one extremity of the embryo to the other. This divides into parts corresponding to the various somites into which the animal divides. These masses gradually separate from each other as the animal increases in size, and concentrate into the several ganglia that form the great nervous chain.

The author also shows the origin of the permanent organs of vision, and the manner in which the number of lenses increases with the growth of the animal, and traces the origin of several of the internal viscera and their mode of growth.

He also figures, in minute detail, the larvæ of an immense number of genera.

An Examination of Dr. Bastian's Experiments.—A very able critique on Dr. Bastian's views is that which was published some time since in the 'British Medical Journal,' by Dr. W. Roberts, Professor of Medicine in Owens College, Manchester. He says:—Dr. Bastian has cited my name and quoted my experiments in a way that might make it appear that my investigations lend some support to his views on the origin of bacteria in organic infusions; this is, however, not the case. On the contrary, the weight of my experiments is entirely against him and in favour of Pasteur's conclusions. I found, as others have done, that some infusions and organic mixtures produced bacteria after having been boiled for a certain time; but I also invariably found that, if the boiling were sufficiently prolonged, no such result followed. All were rendered permanently barren—even Dr. Bastian's favourite turnip-and-cheese mixture. In quoting my experiments on alkalized infusion of hay, Dr. Bastian omitted to add, that in the same paper from which he was quoting, I furnished decisive *experimental proof* (not a mere explanation) that the germination which took place after boiling was due to a survival of preexisting germs, and *not* to a *de novo* generation. The results obtained

by me were in substance identical with those obtained by Pasteur and Professor Tyndall. The circumstance that Professor Tyndall did not encounter those examples of great resistance to sterilization by heat that I encountered, involves no contradiction in our results. His procedure was different from mine, but our results were the same. We both succeeded by boiling in sterilizing our infusions without impairing their aptitude for the growth of bacteria. As well might we say that two chemists contradict each other when they obtain the same metal from the same ore by different processes.

It appears to me that the attitude of Dr. Bastian on the question of the origin of bacteria arises from what I may call the inverted significance which he attaches to the two contrasted results—barrenness or fertility—which follow after boiling an organic infusion. Throughout the controversy, Dr. Bastian speaks of the barren tubes and flasks as "failures" or "negative results"; and he evidently regards the fertile tubes and flasks as "successful" experiments, having the force and authority of "positive" results. The true view is just the reverse of this, and his misunderstanding on this point makes him blind to the overwhelming cogency of the case against him. When the matter is duly considered, it is the barren flask that has the character of a positive result. For what does the experimenter set himself to do in these experiments? He seeks to destroy by boiling all pre-existing bacteria in these infusions, and to leave unimpaired their powers of promoting the growth of bacteria. And it is found in fact that this latter quality is perfectly preserved in boiled infusions; for they breed bacteria with the utmost luxuriance when they are reinfected from an extraneous source. When the experimenter finds that his infusions germinate after boiling, the *primâ facie* probability is, that he has either applied the heat insufficiently, or has permitted extraneous infection after boiling; for this is exactly what would occur if he failed in either of these points. But if the infusions remain barren, this is a new and unexpected consequence, and carries with it the weight and cogency of a positive result. The explanation of the fertile flask is thus ready at hand; it is simply a faulty experiment; but what possible explanation can be given of the barren flask, except that supplied by the panspermic theory? When I take up one of the flasks or bulbs which have remained barren in my chamber for three or four years, though supplied with air (filtered through cotton-wool) and suitable heat, my wonder never ceases. Each one is a new experiment, every day repeated, and multiplied indefinitely; day after day I ask myself, Why does it not germinate? I compare it to a field in spring not yet sown, but ready for the reception of the seed: for if I withdraw the plug of cotton-wool and admit the dust of the air, or introduce a drop of water, all is changed; in a few hours the stillness of years gives place to life and activity. I repeat, it is the fertile flask, and not the barren flask, that wears the complexion of a failure and of a negative result. The reluctance of some evolutionists to give up the spontaneous origin of bacteria is evidently due to the notion that this question is bound up with that of abiogenesis generally. This

is a wholly erroneous idea. The question of abiogenesis will still remain after all have acquiesced in Pasteur's views of the origin of bacteria: indeed, to a logical evolutionist there would appear to be a strong *a priori* improbability in the abiogenic origin of bacteria. They were not wanted, and could not exist, on the earth's surface until after other organisms had lived and died before them. Their special function and feeding-ground lie amid the wreck of living things. And if the survival of the fittest hold good in regard to bacteria, they must be the remote progeny of less perfect organisms of the same class. What can be more perfect than their adaptability to their place and use in the order of nature? They resist, in certain media, for considerable periods the heat of boiling water; they multiply with incredible speed; their germs survive in countless myriads in the dust of the atmosphere; they float in every drop of water on land and sea; they appear to be omnipresent and almost indestructible. Those who are in search of a case of abiogenesis should seek among the primitive organisms—if there be any such—which can exist and grow amid inorganic elements, in the water of the sea, or the mineralized springs and streams of the land. When Pasteur says that abiogenesis is a chimera, he prudently adds, "in the present state of science;" and even thus qualified, the expression is perhaps too strong. But it is absolutely certain that up to the present time no case of abiogenesis has been presented which has stood the test of accurate investigation; nor can it be doubted that, in so far as the antiseptic treatment of disease rests on the origin of bacteria, the advocates of that treatment stand on unassailable ground.

Pollen-tubes for the Microscope.—Mr. J. O'Brien writes to the 'Garden' (August 19) as follows:—"I lately came across a passage in a popular work on microscopy recommending the student who wishes to examine the pollen-tubes of flowers to dissect a fertilizing stigma. Remembering my own early experience in this branch of microscopic preparation, my repeated disappointments after wasting my time in the most tedious manifestation, besides having seen the preparations of others who followed this method of procedure always end in failure, I am induced, more especially at this season of the year, when the opportunities of studying the pollen of plants are so many, to offer a few hints on a new method of observing these beautiful objects. Most persons must have noticed that when the stigmas of Lilium and other flowers have arrived at the period of fertilization, a drop of nectar makes its appearance at their top. This nectar is the one thing necessary for exhibiting the growth of the pollen-tubes. We will take for example the *Lilium speciosum* or *L. auratum*, as they usually produce the most nectar. If a plant or two coming into flower be put into the greenhouse instead of leaving them outside where the flies will help themselves to the nectar, we shall find that in a few days several stigmas will be ready with the drops pendent. Take an ordinary microscope slide and place the centre of it against the most liquid drop, which will remain on the glass; if not sufficient, another drop should be added from another pistil. Then touch the point of

one of the mature anthers with the drop of nectar very gently so as to leave not more than about a dozen pollen-grains on it, and it is ready for examination without even the usual covering of thin glass. If the object be examined directly with a quarter-inch object-glass, or even a lower power, nothing will be seen but the pollen-grains, but in about half an hour a projection like a fleshy root will be seen at the end of each grain, and will continue to grow for from one to two hours, at the end of which time the pollen-grains and tubes will resemble in appearance very long snakes, the grain representing the head. The sap may also be seen running down one side of the tube, turning at the point, and returning on the other side. When prepared in this manner the object is perfect, and being free from foreign matter is clear and beautiful. In preparing the objects care should be taken not to place them in too warm or too dry a place, as the continuous growth of the pollen-tubes entirely depends on the length of time that the nectar remains in a sufficiently fluid state. The student must not be discouraged if he fails a few times, as he will be amply rewarded when he secures a good specimen, which may be made a permanent one without any further trouble than that of placing a piece of thin microscopic glass over it while the nectar is in a fluid state, and carefully pressing it down to eject the air-bubbles. The nectar will soon harden, and the pollen-tubes be preserved in a perfectly transparent vehicle; prepared in this manner, I have a perfect specimen a year old. We may take it for granted that any flower which produces nectar in sufficient quantity will produce the tubes in the manner described; but I have been more successful with bulbous plants than any others, having obtained some beautiful objects from different varieties of Hymenocallis (Pancratium), Crinum, &c. The foregoing is a simple and successful means of accomplishing a delicate operation, which most botanical students, who in these days are invariably workers with the microscope, will be glad to know."

The Minute Anatomy of the Dog's Skin.—This subject has been thoroughly investigated by Dr. W. Stirling, who has published a most valuable paper on the subject in the Reports of the 'Saxon Academy of Sciences' for 1875. He says that the skin to be examined was stretched over a glass ring and digested in artificial gastric juice, prepared by adding 0·2 per cent. of hydrochloric acid to carefully prepared glycerine-pepsine. The temperature was maintained at 38° to 40° Cent. (100·4° to 104° Fahr.). The fluid was renewed every two hours, and digestion was generally sufficiently advanced in four to six hours. The skin, still on the ring, was then washed with water, and placed in distilled water for twenty-four hours. During the time it had swelled to four to six times its thickness, and was in a suitable condition for making sections which could be examined at once, or after being stained. The leg was injected with a clear watery solution of Berlin blue from the femoral artery, with a pressure of 100 to 200 millimeters of mercury, a cord being screwed tightly round the limb above the point of injection. The solution was allowed to flow into the vessel as long as it would go—usually for a period of many hours. By this process the vessels were filled with the blue, whilst

the water passed into the tissues and produced œdema.—In the dog the bundles of fibrillary tissue are mostly parallel to the surface of the cutis, and are held in their position by the elastic fibres which interlace amongst them. There are two kinds of cells in the stroma of the cutis. The nuclei of the one kind are spindle-shaped, of the other round. The cells with round nuclei are mostly found near the vessels, that is, in the superficial part of the cutis and in the subcutaneous tissue. It is suggested from their position that they are lymphatic cells. When digestion is far advanced, part of the *Zell-platte* which belongs to the cell with spindle nuclei, and which the author considers to be analogous to the *Zell-platte* of Schweigger-Seidel, is sometimes preserved. When digestion is not so far advanced, and the arrangement of the fibrillary bundles is undisturbed, it can be seen that the long axis of the spindle nucleus is parallel to the direction of the bundles, and that the spindles lie between the bundles. It is inferred that the clefts between the bundles form spaces which are filled with lymph-fluid. As in the human skin, whilst the hair-follicles, fat-lobules, and sebaceous glands have each a separate blood supply from a small artery, the connective tissue between the fat, muscles, and glands is destitute of capillaries. The sweat-ducts (the popular idea that in dogs there are no sweat-glands is a mistake) open into the hair-follicle above the sebaceous gland, but at some distance from the epidermis. The sweat-gland is composed of a layer of flat cells, a structureless membrane on which these lie, and then the cells of the gland. A similar layer of flat cells has been described in man by Heynold, but he says that they are followed immediately by the gland-cells. The erector pili is composed of elastic fibres, which spring from a network which surrounds the hair-bulb, and which pass in a tract to the surface of the cutis, where they spread out and join the other fibres of the part. Between and amongst these elastic fibres, the smooth muscle-cells are insinuated. After prolonged digestion the bundles of fibrillary tissue undergo a change by which they present an appearance like that usually presented by muscular fibres. This is due to the gelatinous substance being dissolved, a sheath remaining, which, being thrown into transverse folds, simulates the transverse markings of muscle. Both the larger and the very fine bundles have such sheaths. When digestion is continued until the skin is reduced to a pulpy mass, what remains of the capillary blood-vessels is a very delicate structure composed of rows of spindle-shaped cells.

NOTES AND MEMORANDA.

Illumination in connection with Polarization.—There is a capital paper on this somewhat complex subject in the 'Journal of the Quekett Club' for September, 1876. It is, however, too long for insertion as a whole, and it is very difficult to abstract. We observe also that the author has appended a note to the effect that he has succeeded in adapting the principle of the side reflector as a *sub-stage arrangement for transparent objects*, which possesses all the merits of the Bramhall mode of illumination without any of its defects; and combines also the additional advantage of being applicable to general purposes (which the other is not), producing a beautifully soft and clear light, with great perfection of definition. It is so arranged that the angle of incidence may be regulated at will and set at any degree of obliquity capable of producing from a dark-ground illumination to almost direct rays; whilst it admits of the lamp being placed in front of the instrument, so as to be entirely out of the way of the hands and face.

A New Mode of Mounting Foraminifera is given by a very distinguished authority, Professor H. L. Smith, of Hobart College, New York. It is published in the 'Journal of the Quekett Club' (September), and contains besides the description of the above mode an account of a somewhat similar method for the Diatomaceæ. For the Foraminifera he punches out of a sheet of wax (dark-green or black), a disk a trifle larger than the brass curtain-ring which is to constitute the wall of the cell. This disk is pressed by one edge to the centre of a glass slide, and slowly warmed till it melts—if well done no bubble of air is enclosed under it, and the whole cools with a smooth, somewhat dead surface. The ring is then pressed into this, and centered by the turn-table, and then again pressed fully home, showing the brass, when looked at from the under side; and the whole finished with the usual "Brunswick black" outside, and also the ring inside. To attach the Foraminifera, or other objects, a *minute* drop of turpentine is applied to the wax, and in a minute or so, before it is quite dry (and we may proceed leisurely), the object is placed on the softened wax; when thoroughly dry, it will be found so strongly attached that a violent blow or a fall will not dislodge it. Of course, if the object is very large the turpentine may have a little of the Brunswick black or some size dissolved in it. The improvement over the glistening gum attachment for minute objects is very manifest—indeed no signs of the cementing material show if the turpentine is judiciously used. While all this is being done, the Brunswick black on the brass ring will have set sufficiently to fasten the cover, which should be of such a size as to rest, not on the top of the ring, but to slip just within, so that its surface will be flush with the top of the ring. When the cover is pressed home, the whole may at once, without any danger of its "running in," be finished with the black varnish. Nothing can exceed the soft and

delicate appearance of these wax backgrounds, nor can a cell be built up in any manner more readily. I use the sheets prepared for wax flowers, and of course colours may be selected to suit the object. The disks are punched by a solid plunger, which must frequently be cleaned by punching disks from a thick card either oiled or prepared with a little turpentine. Care will enable one to prepare these rapidly.

Chloral Hydrate as a Medium for Mounting.—Mr. T. S. Ralph writes from Melbourne to 'Science Gossip' of October, advocating this substance as a medium for mounting. He says that this chemical compound will dissolve and unite perfectly with many substances, and from some of its combinations we may obtain mediums which may vary from a fluid to a jelly-like or gum-like consistence. Chloral hydrate, besides uniting with gum and resins, also unites with alkaloids; as salicinine, quinine, and cinchonine. "Place a small portion of chloral on a glass slide, add a little portion of water, and a nearly equal bulk of either of the above; gently heat over a flame, mixing the two materials with a needle or glass rod: these will unite, and when cooled be found to be viscid and clear. To such a mixture camphor can be added, or glycerine. If a number of such experiments be conducted, the operator will soon find out what he is likely to obtain, and thus contrive a new medium suited to some objects. I propose the following:—Make a nearly saturated solution of chloral in water; filter, and then add sulphate of cinchonine to near saturation. A portion of this combination is placed on a slide, heated slightly and allowed to cool. The object is then placed in it and the cover applied, which, however, requires to be cemented with gum solution, and then Bell's cement or Canada balsam. To this 'artificial balsam,' as I am inclined to call it, I have added a little dextrine, so as to render it more solid. Your workers can make trial, and report on it. Glycerine and dextrine together boiled, and then chloral hydrate added, make a clear solution, which, I think, may be tried also."

CORRESPONDENCE.

Dr. Woodward's Photographs of Frustulia Saxonica.

To the Editor of the 'Monthly Microscopical Journal.'

Wayland Depôt, N.Y., *August* 11, 1876.

Dear Sir,—I have this moment read your remarks in the August number on my note on "The Markings of *Frustulia Saxonica*" There are, of course, many things in the way of obtaining maximum performance. Perhaps my remark about careful adjustment, &c., may do Dr. Woodward an unintentional injustice. I do not wish to be understood as saying, nor do I think that Dr. Woodward has not a good glass and is not a skilful manipulator; but every observer understands

that there are favourable times and conditions when the best work, obtained under ordinary conditions, may be surpassed; and, further, that a very little gain in adjustments, quality of glass, illumination, &c., often presents the object, as it were, in a new light. It would, indeed, be wonderful if anyone had succeeded in meeting all the requirements and preserving the results in a photograph. I may still insist upon it, that his photographs of the diatom in question, however excellent they may be as compared with others, do not represent the best work of the best glasses under the most favourable conditions, and that, therefore, negative statements as to its structure, even when coming from so high an authority, are not so good as positive ones. Analogy would indicate that the structure of *F. Saxonica* (probably a variety of *N. rhomboides**) is the same as that of the large specimens of *Navicula rhomboides.*

As to whether or not the diatom examined by me is the one generally known by the name of *F. Saxonica*, a few words may be necessary. I have long thought *F. Saxonica* to be only a variety of *N. rhomboides.* I have slides, and a fine gathering of material, containing three sizes of *N. rhomboides*, the largest easy for a dry $\frac{1}{4}$th, and the smallest not as difficult as the variety mounted by Möller, and called *F. Saxonica.* The only diatoms I have ever spoken of as *F. Saxonica* are those furnished by Möller, and about equal in difficulty of resolution, the No. 18 of his Probe-Platte, and if they differ from that at all, the difference is slight.

As further evidence that the best work of the high-angle objectives has not yet been *photographed*, it may not be out of place to say that the best photographic showing of the transverse markings of *Amphipleura pellucida* that I have yet seen was obtained by Dr. Charles Jewett, of Brooklyn, with a Tolles' immersion $\frac{1}{5}$th, made four years ago, angle of aperture 115° at uncovered.

I feel confident that none of us have yet seen the best work of our finest instruments. Much is to be gained by improved methods of illumination, patience, and "painstaking manipulations." We must, as was once remarked by Dr. Pigott, be sure that we are in best condition for work, and prepare ourselves for it. Then we should devote days to a study now passed off at a single sitting.

Very truly yours,

G. W. Morehouse.

Errata in Dr. Johnston's Paper.†

To the Editor of the 'Monthly Microscopical Journal.'

Baltimore, *August* 12, 1876.

Sir,—Will you do me the favour to call attention to the following *errata* in my recent paper on the "Spermatozoa of Amphiuma": On the first page, *undulations* instead of modulations; *formed* for rounded.

* See recent articles by Mr. Kitton, Professor Smith, and Mr. Stodder, in this Journal.

† These errors seem to be unpardonable, but if the reader saw Dr. Johnston's handwriting he would consider them very trifling indeed.—Ed. 'M. M. J.'

On the second page, read "ended *in* the vague." On the third, *undulating* again for modulating; *Czermak* for Azermath; and, lastly, substitute $\frac{1}{343}$ for $\frac{1}{383}$.

In note, "*my*" should be "our."

Very respectfully,

CHRISTOPHER JOHNSTON, M.D.

PROFESSOR ABBE ON DR. PIGOTT'S PAPER "ON THE PRESENT LIMITS OF VISION."

To the Editor of the 'Monthly Microscopical Journal.'

JENA UNIVERSITY, *October* 9, 1876.

SIR,—In the October issue of the 'Monthly Microscopical Journal' I find a paper by Dr. Royston-Pigott, "On the Present Limits of Vision," from which I understand that Mr. Helmholtz and myself have "popularized" a doctrine of Lagrange about the limits of vision, as deduced from the wave-principle. In particular, Dr. Royston-Pigott, p. 180, quotes as "deduced from those of Lagrange" a formula which is the keystone of our theory, establishing the exact relation between the limits of visibility and the angular aperture of an optical system.

I myself till now was not aware of this connection with Lagrange, nor will Mr. Helmholtz be, I am sure. I was of opinion that the said doctrine as a whole, and the quoted formula in particular, had been advanced by me for the first time in 1873 in Max Schultze's 'Archiv,' and a short time afterwards—quite independently and from a different point of view—by Mr. Helmholtz in the 'Proceedings of the Berlin Academy' and in Poggendorff's 'Annalen.'

To my knowledge there is only a theorem by Lagrange of a purely geometrical character, which Mr. Helmholtz quotes and takes as a starting point in his deduction; but this theorem has no relation at all either to the wave-principle or to the diffraction theory, and, besides, is essentially limited to the supposition of infinitely small apertures. For both reasons this theorem—either by itself or with all the other propositions in Lagrange's paper "Sur une Loi générale d'Optique"—is manifestly insufficient to form any conclusion aiming at the limits of vision; for such a conclusion involves a complete theory of the diffraction effect in optical systems, and at the same time a proposition about the convergence of pencils in aplanatic systems of finite (i. e. great) apertures.

Now, from Dr. Royston-Pigott's decided assertions, I must infer, as many readers of the 'Monthly Microscopical Journal' will do, that in some paper hitherto ignored by other writers Lagrange has already expressed a clear notion of the said problem in general, and, besides that, has treated the special questions upon which the solution essentially depends.

Since the doctrine in view will prove more valid than Dr. Royston-Pigott now seems inclined to concede, and in future, I think, will be considered as of some importance in the theory of optical instruments, many readers of the 'Monthly Microscopical Journal' will take interest in a clear statement of its origin. On that account I hope you will grant a place in your Journal to these remarks, and thereby induce Dr. Royston-Pigott to reveal his knowledge of the hitherto unknown writings of Lagrange he refers to, in kindly naming paper and page, where Lagrange has applied the wave-principle to the problems of vision,—has investigated the dioptrical performance of high-angled systems,—and has advanced the principles from which the formula quoted could be derived.

I am, Sir, yours obediently,

Dr. Ernst Abbe.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *October* 4, 1876.

H. C. Sorby, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of presents to the Society was read, and the thanks of the meeting were voted to the donors.

A paper was read by Mr. Thomas Palmer, "On a New Method of Measuring and Recording the Bands in Spectra," the subject being illustrated by drawings placed in the hands of the President, and by the exhibition of the apparatus described, showing the application of the micrometer scale to the spectrum of a solution of nitrate of didymium. (The paper will appear in our next number.)

The President proposed a vote of thanks to Mr. Palmer, which was carried unanimously. He expressed the pleasure which he felt at finding that some further attention was being given to this subject, and thought he need hardly say that he had listened to the paper with very great interest. There were a number of points in it which were worthy of notice, and he could not help saying that he was exceedingly pleased that Mr. Palmer had seen the desirability of reducing his measurements to wave-lengths. He judged, from the descriptions given, that the scale would have to be altered if a different prism were used, and asked Mr. Palmer if that were not so.

Mr. Palmer said that of course the actual measurements would only apply to the same prism, or to one with the same refractive index.

The President said it appeared to him that in every case they

should work, or endeavour to do so, upon some uniform system of measurement, such as that of wave-lengths. With regard to the advisability of using a cap over the front of the objective, he had found its advantages to be so great that he now considered everything he had done in that way before using one to be worth nothing at all. In working without a cap, he found that a spurious absorption band was obtained, which varied very much according to the strength of the solution under observation, and this difficulty was not only entirely got over by using the cap, but the actual bands of the spectrum were seen much more sharply than before. A cap of this kind would also be found of the greatest possible advantage in working without the spectroscope, because by cutting off extraneous rays in that way, in the examination of crystals for instance, the true colours could be ascertained far more perfectly than was otherwise possible. He was very pleased indeed to find that others were working in that field, and he congratulated Mr. Palmer upon the results he had already obtained. He had been working lately, and carrying on some experiments, the results of which he hoped to lay before them on some future occasion, and amongst other things he had thought that in future the slit should be made to act symmetrically, moving from both sides at the same time, instead of at one side only. In some cases, also, he found it would be better not to make the bright dot move, but to make the spectrum itself move, so that the part it was desired to examine might always be brought into the centre of the field. These alterations would overcome a great many objections and difficulties which had hitherto been felt in the way when working with the micro-spectroscope.

Mr. Palmer said that with regard to the use of the cap, some time ago he made up a rather strong solution of permanganate of potash, which gave, when properly shown, five distinct bands; but with this, as seen without the cap, the whole spectrum was completely "fogged," and the space occupied by the five bands was just one great black "splodge," and all the rest was cloudy; but when the cap was put on, it broke up into bands as sharply as possible.

Dr. Pigott inquired what power objective was used in making these observations, and being informed that it was 2 inches, he said the cap no doubt reduced the aperture, and he thought the smaller the aperture the more it would approximate to a telescope. It was an interesting question, whether the contraction of the aperture was the cause of the increased distinctness.

The President said the advantage of the cap was not that it reduced the angle of the glass, but that it prevented the reflexion of the light from the face of the glass covers back upon the object-glass.

Dr. Pigott said he had not quite understood where the cap was used; he thought in one case it was placed above the objective. It was an interesting point whether it would not be well if placed above the lens of the objective.

Mr. Ingpen said that the effect of the cap was entirely dependent upon its position; if put near the lens it would reduce the aperture, but it would not do so at all if it were placed at the focal distance in

front of the lens. Of the two positions, it would be far more beneficial when placed in contact with the object.

Dr. Pigott thought that one other point was important—he believed it was stated that the measurement extended to the $\frac{1}{2500000}$ inch.

The President said that the scale was divided, so that the tables enabled the reading to be made of millionths of millimeters, and the decimals would be ten millionths. His own impression was, however, that they could not be certain of anything less than the millionth of a millimeter.

A paper "On the Microscopical Structure of Amber," by Mr. H. C. Sorby and Mr. P. J. Butler, was read by the President, who illustrated the subject by numerous drawings enlarged upon the blackboard. (The paper will be found printed at p. 225.)

Dr. Pigott said, with regard to the included globules, he was engaged some time since in examining the refractive index of globules, and he found that the stronger the refractive index, the smaller was the central spot seen. There was a very beautiful mathematical relation between the nuclei shown by water, or gas, or other fluids.

The President said he much regretted the absence of Mr. Butler that evening, because it was to him that the merit of the paper was chiefly due for the great number of sections which he had prepared, and the amount of time which he had devoted to them.

A vote of thanks to Mr. Sorby and Mr. Butler was put to the meeting by Mr. H. J. Slack, and carried unanimously.

The Secretary said they had received a paper from Dr. William Hinds, of Birmingham, "On a Curious Fact in connection with certain Cells in the Leaves of *Hypericum androsæmum*," but owing to the lateness of the hour it would not be possible then to read it. The contents of the paper were stated, which was "taken as read." (See p. 233.) He also reminded the Fellows that they had received some time ago a quantity of specimens of minerals and diatomaceous earth from Mr. Hanks, of San Francisco, and that the earth had been sent to Mr. Kitton for examination. That gentleman's report would be published in the Journal (see p. 232), and he thought would prove interesting, as tending to reduce the number of species of *Aulacodiscus*, &c., by showing that the furrow had little value as a specific character. He had written to Mr. Hanks for a further supply of this earth, and if it existed in large quantities, and it were possible to send them several pounds weight of it, an examination of so large a quantity would probably result in a still further reduction.

The Secretary called attention to a section of a so-called "Brighton pebble," exhibited under a microscope in the room, and containing small objects, resembling those figured by Dr. Duncan in the May number of the 'Quarterly Journal of the Geological Society.'

It was also provisionally announced that, subject to permission being obtained for the use of the rooms, a scientific evening would be arranged by the Council for the second Wednesday in November.*

* The permission was subsequently obtained.

Donations to the Library since June 7, 1876 :

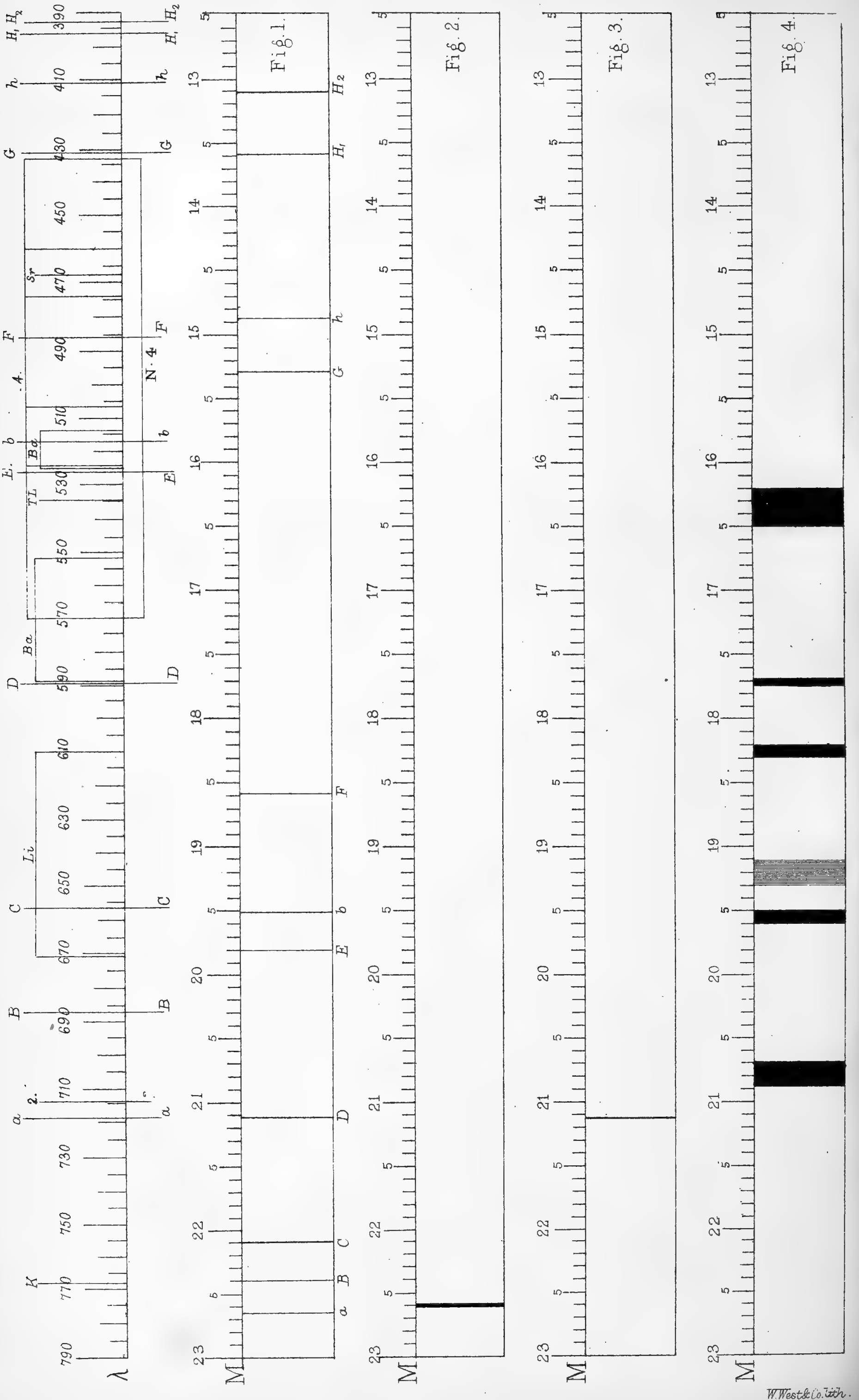
	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*Society.*
A Manual of Microscopic Mounting. By John H. Martin	*Author.*
Quarterly Journal of the Geological Society. Nos. 126 and 127	*Society.*
The Journal of the Linnean Society	*Ditto.*
The Journal of the Quekett Club	*Club.*
Proceedings of the West London Scientific and Field Club	*Secretary.*
Bulletin de la Société Royale de Botanique de Belgique	*Society.*
Bulletin de la Société Botanique de France. 3 Parts ..	*Ditto.*
The American Journal of Microscopy and Popular Science. 8 Parts	*Editor.*
Verhandlungen des Naturhistorisch-Medicinischen Vereins zu Heidelberg, 1874–5–6.	
Popular Science Review. Nos. 60 and 61	*Editor.*
Medical and Surgical History of the War of the Rebellion. Part 2nd, Surgical Volume	*Surgeon-General, U.S.A.*
Half-a-dozen Photographs of *Navicula rhomboides*	*Ditto.*
The Application of Photography to Micrometry, with special reference to the Micrometry of Blood in Criminal Cases. By Dr. J. J. Woodward. Illustrated with Photographs	*Author.*
Further Notes on Inclusions in Gems, &c. By Isaac Lee, LL.D.	*Ditto.*

Frederick Kitton, Esq., of Norwich, was elected an Honorary Fellow.

[M. Coruet, Secretary of the Société Belge de Microscopie, was introduced to the President, and welcomed in the name of the Society.]

WALTER W. REEVES,

Assist.-Secretary.



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THE

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I.—*On a New Method of Measuring and Recording the Bands in the Spectrum.* By Thomas Palmer, B.Sc.

(*Read before the* Royal Microscopical Society, *October* 4, 1876.)

Plate CLXI.

The attention of workers at the micro-spectroscope has been now for some considerable time engaged in endeavouring to find out some more practical method of measuring and recording those spectra which are seen: to meet all the numerous requirements seems almost impossible. Taking, however, into consideration the short space of time that has elapsed since the adaptation of the spectroscope to the microscope, and the extreme difficulty which the whole subject presents, you will all perhaps agree with me that a great deal has been done; in some instances, I am afraid, however, in a somewhat mistaken direction, for a measure, if it is to be worth much, must act alike in everyone's instrument, so that a certain formula may be set down for each band in question. This, I am quite aware, is a very high standard of perfection; but that can surely be no reason for asserting that it is not to be reached. Many of you will agree with me when I say that our worthy President's, Mr. Sorby's, new method is near, at least at present, to that point; but the quartz block, as perhaps you all know, is so extremely difficult to get right; still the principle is correct, viz. that though a position is taken by the micrometer, the reference to and actual measurement of a band is in wave-lengths. This is what struck me just after I had heard Mr. Sorby's paper, and, besides endeavouring to get one of these measures made for myself, in which I need hardly say I have failed, I set to work to try if possible to do something for this branch of research; the few remarks, therefore, which I now beg to offer I hope will meet with your full approbation.

The class of instrument that I have employed throughout is an ordinary Sorby-Browning micro-spectroscope; with respect to the measure, I have adopted a fitting somewhat like that used by Mr. Browning in his bright-spot micrometer, but with this

exception, however, that instead of a spot in the cavity of the hollow tube which carries the mirror, I have in my case placed in an angular slot a rather dense negative photograph of a millimeter scale, divided into tenths, which serves as a micrometer; the lines are therefore produced as spaces, through which the reflected rays from a small mirror pass, thence through two double convex lenses, which combination is capable of being adjusted for the purpose of focussing the image, which is then reflected upon the prism in such a position that it falls just above or below the spectrum; this is obtained by moving the eye-piece fitting, and a small pin is placed in a slit at the side so as to prevent it from going too far.

I then found that I must either reduce the scale from its present size, or put up with distortion, a difficulty which is quite overcome by the means I have adopted, viz. to elongate the tube at the back of the scale, to use a larger and longer focus lens, and an angular slot instead of a round hole, through which the incident rays pass; by this contrivance aberration and distortion are avoided, and the whole of the scale is seen quite clear and distinct, which fact appears, at least to me, to be a very important point in its favour.

The adjusting screw, to which is attached the milled head scale, is used for setting the micrometer when necessary; this has seldom to be altered, as it will answer for nearly all observations. It may, however, at times be useful to have the movement, more particularly for the purpose of arriving at the centre of a band. Care should be taken always to put it back to its previous position, which is easily managed by noticing the milled head, one revolution of which is equal to five divisions of the micrometer scale. (See Table 1.)

So much for the piece of apparatus. We will now proceed to consider briefly the manner of using it, previous to commencing upon the actual measurement of bands by its means. To ensure success, therefore, I must first of all lay down a few rules, which I have divided into four heads, as follows:

Firstly. Before proceeding to make an observation, arrange your light, focus the slit carefully, and put on the spectroscope, when the spectrum ought to be quite distinct; then, by means of the small mirror, illuminate the micrometer scale, and focus it till the lines appear quite sharp and free from blur; avoid a glare, or this is sure to be the case, but try to get as white a light as possible.

Secondly. Care should be taken that the lines of the micrometer are quite parallel with the bands seen in the spectrum.

Thirdly. As the size of the slit greatly influences the appearance of the bands in the spectrum, it is necessary to modify it

according to circumstances, which will be fully considered presently.

Fourthly. It is as well always to work with the micrometer, though it may not be your intention to use it; it should be set by means of the adjusting screw to the position of some known band, and the division carefully recorded.

The method I have adopted for measuring these bands is as follows: *Firstly, as those which are symmetrical,* or equally shaded to the right and left; *secondly, as those which are unsymmetrical,* and which are unequally shaded to the right and left.

Of all symmetrical bands, it is best to measure directly for the centre, for, after a little practice, it is surprising with what exactitude the eye perceives the slightest inequality between the two halves of an object which in itself is quite symmetrical; it is, therefore, I find, easier to place a division of the micrometer on the centre of any band at once, be it broad or narrow, than to estimate it by measuring from the two ends and so deducting. In the case of those very feeble bands of this class, it is advisable to move the micrometer alternately from left to right, and right to left, stopping when you have succeeded in getting a line exactly in the centre of the band. This movement of the scale will, I think, be found very serviceable in facilitating a correct result.

When, as in the case of unsymmetrical bands, they are shaded throughout, that is to say from one end to the other, I measure the end which is the most marked; and when the band appears nebulous, I take the centre. In case neither of the ends are distinct enough to allow of any definite accuracy in determination, or in the case of those very feeble bands which, in some instances, are scarcely visible, I take the apparent centre as near as possible, in which case the measuring is subjected to the following causes of error:

Firstly. The eye of the observer is inclined to take for the centre not the actual one, but a point which is more or less allied towards the maximum of the light.

Secondly. When the luminous intensity, or the opening of the slit increases, the unsymmetrical bands are unequally enlarged, or drawn out towards the ends.

Thirdly. Clear terminations, as in the case of all symmetrical bands, remain relatively fixed, whilst those which present a feeble, undecided appearance change from their original positions; consequently the true centres become displaced.

Fourthly. These centres, as well also as the whole spectrum, are materially changed by the thickness or strength of the substance or solution under observation. This effect may be better

seen when the solution is acid, or alkaline; as, for instance, in the spectrum of the colouring matter of alkanet root, shown by Mr. Sorby in his interesting paper on "New and Improved Microscope Spectrum Apparatus, and on its Application to various Branches of Research."* Also another somewhat remarkable change occurs when, by reason of the light reflected through it, the object under observation becomes warm, those rays which are the most refrangible nearly always gain in intensity. Nitric totroxide is a good example of this.

With regard to those bands which are situated in the red and violet extremities, when measuring them it is as well to direct the line of vision so that one sees on the right of such a band equally as many divisions of the micrometer as on the left, taking care, at the same time, that the eye is placed as near to the eye-piece as possible, and kept in one position, or the original position of the band may appear to sustain an alteration. Again, as we have previously seen in Rule 3, when the slit is too open, or the source of illumination too strong, the bands become distorted; for the purpose, therefore, of testing the accuracy of your measurement, it is as well to make an observation on some other band, which is known to be situated in the vicinity of the one you were working on.

I have likewise been induced, through the ardent researches of our President, to reduce my measurements into wave-lengths, and having succeeded in working out the divisions of my micrometer to that scale, I have, as will be seen, expressed them on my drawings by the Greek letter λ, whilst the micrometer is notified by the letter M. By this method I find that the wave-length corresponding to the position determined by the micrometer, as the centre of a large, well-defined band, is not exactly the mean wave-length of the two extremities. For instance, note the increased size of the wave-lengths in the violet end of the spectrum to those in the red: the left half of the band seen in the spectroscope shows, therefore, more divisions of wave-lengths than that half to the right.

In the description of the micrometer-scale divisions I have employed a decimal, but when at work it is more convenient to use a fraction of some sort, such as for instance, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{10}$, &c. Abbreviations and signs, instead of words, which would take too much time to write in full, are very useful whilst making an observation; thus, to denote all well-marked bands, or those of the first class, viz. symmetrical, I make use of an M; whilst for those of the second class, or unsymmetrical ones, I use an *x*. Then, again, by reason of the milled head, I am enabled to measure with it and the micrometer scale combined to the ten-thousandth of a

* 'Royal Microscopical Journal,' vol. xiii. p. 198.

millimeter, which is nearly the extreme limit of any reliable exactitude. Table No. 1 shows the equivalent value of the milled head divisions in their relation to those of the micrometer, and which must be added to the primary reading thus:

Band situated between 22·7 and 8 of the micrometer, milled head records when turned to get the centre 5, then

$$22{\cdot}7 + 0{\cdot}025 = 22{\cdot}725;$$

these milled head divisions may be again divided to a $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, or $\frac{1}{10}$, in which case the divisions of Table No. 1 would have to be divided by the fraction employed. In this paper I have avoided entering upon any very practical results, as I wish it to be exclusively one on a new style of measurement; however, I will, with your permission, say in as few words as possible, which bands I have taken, as those to which reference may be made. They are as follows, and comprise the principal lines of the solar spectrum:

See Plate CLXI., Fig. 1.

	M.	λ.	
a Solar	23·35	718·5	Angström.
B "	23·6	686·7	"
C "	23·9	656·2	Mascart and Angström.
D "	22·87	589·2	Fraunhofer.
E "	20·2	526·9	Angström.
b "	20·48	517·2	"
F "	19·42	486·1	"
G "	17·72	430·7	"
h "	15·13	410·1	Thalen.
H_1 "	14·42	396·8	Angström.
H_2 "	14·87	393·3	"

For the purpose, however, of establishing a standard for evening work, I have not used these Fraunhofer lines of the solar spectrum, but have taken spectra which are readily produced by simple means, so that when the measurement of a band is required to be checked that substance which is known to give a band near to the one in question is placed on the side stage; the two spectra are therefore compared, and the distance from that known band is readily determined. These I call my test-objects, they are all of the absorption class of spectrum, with the exception of sodium, which I have obtained by burning a little common salt in the flame of a Bunsen burner.

The test-objects employed are as follows, and I should perhaps add, before proceeding to the measurements of these standard spectra, that in all cases of observation I always use a small cap

over the tube which contains the objective, which has a hole, the one-sixteenth of an inch in diameter, cut in it. By this arrangement all extraneous light is prevented from passing up the body of the microscope, except what passes through the object. I have tried examining objects both with and without this small piece of apparatus, and am quite convinced that unless this precaution be attended to, a false result is frequently obtained; this, however, I am quite sure of, that it tends to assist the observer greatly in making correct measurements.

No. 1. See Plate CLXI., Fig. 2.

Oxalate of Chromium and Soda.

This solution is of a blackish purple colour, and is remarkable for the sharp narrow band below B, in the red; the others in the yellow and violet I have discarded as not being serviceable to my requirements. It is almost opaque by daylight, and shows no bands, it must therefore be viewed by the aid of a lamp.

M.		λ.	Observations.
23·407	Middle	711·75	Very black band, ¼ of a division in size, ends, sides, and centre equal.

No. 2. See Fig. 3.

Sodium

is an incandescent spectrum, and expresses D solar or the line produced by the combustion of sodium in the Bunsen or spirit-lamp flame; it is easily detected owing to its position, which is in the centre of the yellow.

M.		λ.	Observations.
22·875	Middle	589·2	Bright well-defined band, in the yellow, size about ½ a division.

No. 3. See Fig. 4.

Nitrate Didymium (or Didymium Nitrate).

Owing to the extreme sharpness of the absorption bands in this spectrum, it is the one I invariably use as a standard to determine the position of others by.

M.		λ.	Observations.
1. 21·23	Middle	568·9	Sharp, very black, 2 divisions in size, ends and centre equal. Near D.
2. 20·45	„	517·25	Sharp, black, 1 division in size, ends and centre equal. Near E.
3. 20·79	„	504·75	Sharp, shaded, $1\frac{4}{5}$ divisions in size, ends and centre equal. Near *b*.
4. 19·77	„	474·25	Sharp, shaded, $\frac{1}{2}$ division in size, ends and centre equal. Near F.
5. 18·32	„	460·00	Sharp, shaded, $1\frac{2}{5}$ division in size, ends and centre equal.
6. 17·65	„	432·12	Sharp, black, 3 divisions in size, same as No. 1. Near G.

And now with respect to mapping the spectra and drawing the charts, the plan which I have adopted is a very simple one; thus I first of all produce an exact facsimile of what is actually seen, as compared with the result of some different substance, somewhat approaching it in resemblance or otherwise, then by means of the micrometer scale produce the bands in a succession of lines, the thickness of which is shown by the brackets, you have then only to refer to the tables and to affix the wave-lengths, in which case, as seen on again referring to the drawings, they are marked on the top, whilst the micrometer divisions are recorded below. The absorption bands which are shown on the top of this scale represent those of the first spectrum or that of the object on the stage of the microscope, whilst the underneath are those of the second, or the spectrum of the object on the side or comparison stage.

And now what has taken many months of hard work has been expressed in a few minutes, I hope to your satisfaction. There has of necessity been some repetition, but I thought it advisable to lay down in an explicit manner the main principles as to how I work, that similar results, with equal success, may be obtained by other observers in this most interesting and, I am afraid, somewhat neglected branch of microscopy. However, my purpose in ending thus is, first of all, to thank you for the kind attention you have given me; and, finally, that I may retire with the hope of being

again able to bring before you some really valuable work, performed by this new method of measuring the bands in the spectrum.

TABLE No. 1.—VALUE OF THE MILLED HEAD DIVISIONS.

1	0·005	6	0·030	11	0·055	16	0·080
2	0·010	7	0·035	12	0·060	17	0·085
3	0·015	8	0·040	13	0·065	18	0·090
4	0·020	9	0·045	14	0·070	19	0·095
5	0·025	**10**	**0·050**	15	0·075	**20**	**0·100**

II.—*On the Measurement of the Angle of Aperture of Object-glasses.* By F. H. Wenham.

(*Read before the* Royal Microscopical Society, *November* 1, 1876.)

All methods hitherto employed for measuring the degrees of the angle of aperture of microscope object-glasses, have included rays from oblique pencils which constitute the entire field. These having greater divergence from the axis and being superadded to the angle proper, confuse angle of aperture with angle of field.

This remarkable source of error appears to have escaped notice, and is sometimes so considerable as to show an excess in the highest powers of nearly twice the amount of aperture attributed to them by opticians.

The angle of aperture in reality means, that the cone of rays should proceed from one approximate point in the field of view: for example, it cannot be doubted, that if the margin of a very minute diatom were to be enclosed by a screen impervious to light it would not only be seen with the full aperture, but also with improved definition on account of the exclusion of extraneous rays from the field of view.

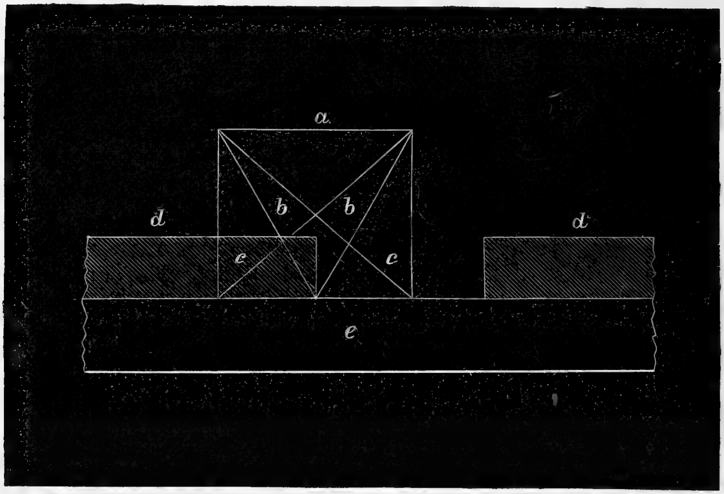
It is the admission of a portion of the rays from lateral pencils that gives an erroneous excess of aperture, by the usual methods of measurement, for the marginal rays of the lateral pencils having a greater obliquity than the outer rays of the central one, of course admit light beyond the true degree, and therefore they should be stopped off during the measurement.

In order to confine the cone of rays to an approximate point in the focus of the object-glass, I have proposed that the light should be admitted through a very narrow slit cut in an opaque film duly set in the focus. There are, however, several difficulties in the use of this slit. In the first place, the measurement depends on the effect of the disappearance of light. This is not altogether a definite indication, and may give rise to dispute, as it may be asked whether the index is to be stopped at the first movement in the slit, or continued till the last trace of light remains visible? Secondly, it may be objected that however thin the material through which the slit is cut, if the space is exceedingly narrow very oblique marginal rays from a large aperture may be cut off.

If the first objection is met by obtaining through a suitable eye-piece arrangement a telescopic image of the lamp flame, in order to see this distinctly the slit must be opened out to a width that will cause a considerable error in excess of spurious aperture, arising from the admission of some lateral rays.

I have stated that "the narrower the slit, the more accurate the result will be." This means strictly that for absolute accuracy, we

must approach to a line, and cut off all rays in the focal plane on either side, quite up to the axis of the object-glass. To ensure this condition I now adopt the following method of measuring apertures:—*a* is the working diameter of an object-glass; *b*, the central pencil, or true angle of aperture; *c*, *c*, oblique or lateral pencils enclosing the field of view; *d*, *d* is a slit of considerable width, with parallel edges attached to a glass slip, *e*. In order to measure



apertures, the object-glass is first adjusted and focussed on the upper surface of the glass slip. One edge of the slit is now brought forward so as exactly to bisect the field of view, half of which will appear quite dark. Over the eye-piece is now placed a cap containing a biconcave lens of about half an inch radii; by means of this and the movement of the sliding containing tube, a distinct telescopic image of a distant lamp, or other bright object, may be obtained through the open half of the object-glass. Turn the open end *away* from the lamp by rotating the microscope, and the flame will suddenly disappear at the point when it is obscured by the edge of the slit. Mark this as zero! Now remove the lens from over the eye-piece, bring back the slit till the opposite edge obscures the other half of the field, and again exactly bisects it, seeing that plane *e* is still in focus, replace the cap and turn the microscope, till the flame again vanishes, and the true aperture will be indicated.

It will readily be seen by the diagram how the rays, *c*, *c*, of the oblique pencils, which have hitherto given a false indication in excess of aperture, are cut off by the edges. The thickness of these by this method is of no consequence, as it is the bottom of

the edge only that intercepts the extraneous rays. The top might touch the object-glass without detriment to the result. However, it is more convenient to employ a film as thin as possible for the slit. This may be easily made as follows:—Take an ordinary 3×1 glass slip, pour upon it a drop of turpentine (which is best after being long kept), and drain it off; move the slide about over the open smoking flame of an ordinary coal oil lamp, till the black deposit is quite impervious to light. Again pour turpentine over the cooled slide, drain, and evaporate it dry by heat. The film may now be cut to a very clear edge with a sharp penknife, drawn along the edge of a small square. Make two cuts about one-twentieth of an inch asunder, and scrape away the intermediate black with a pointed wire, guided by the straightedge. Lay over the slit a thin glass cover, and let Canada balsam run under it by capillary attraction. The small particles left in the slit are excellent objects to adjust the object-glass by, the aperture of which is taken in the conditions of its actual use, and for an immersion lens water can be used between that and the cover.

It is not difficult to bisect the field by estimation, but if required a cross line may be inserted in the micrometer slit of the eye-piece.

The plan of obtaining an angle of aperture by measuring the working portion of the front lens by the diameter of the spot of light transmission, and distance of focus from the surface as the data for the angle, is only so far useful for demonstrating that certain stated angles are impossible, for the computed result may be excessively inaccurate. For example, an object-glass may have an extensive field with good definition with no focal distance, the plane of this being on the surface of the front lens. In this case, of course, the resulting angle will come out near to 180°, however small it may in reality be.

Finally, in order to show the fallacious value of pretended angles near to this extreme, I append a table of apertures: the relative value of the degrees is taken as the chord of the arc. The limit of 180° is indexed as 100, and the corresponding figures give the comparative percentage of value for each aperture.

Relative value of angular aperture taken as the Chord of the Arc.			Relative value of angular aperture taken as the Chord of the Arc.		
180°	=	100·	130°	=	90·6
170°	=	99·6	120°	=	86·6
160°	=	98·4	110°	=	81·9
150°	=	96·5	100°	=	76·6
140°	=	93·9	50°	=	42·2

By this table it will be seen that there is but little to be gained by apertures exceeding 150°, while the last lines of the column show the great increase of value for a corresponding increase in degrees.

III.—*Experiments with a Sterile Putrescible Fluid exposed alternately to an Optically Pure Atmosphere, and to one charged with known Organic Germs of extreme minuteness.* By Rev. W. H. Dallinger, V.P.R.M.S.

(*Read before the* Royal Microscopical Society, *November* 1, 1876.)

By a series of carefully-conducted and well-controlled experiments, Professor Tyndall has recently shown * that sterilized organic fluids which have been filtered, will remain sterile for an indefinite time while they are kept in an atmosphere optically pure. It is well known that a beam of strong sunlight, even in diffused daylight, clearly reveals its path in the air; but if in darkness, the intense beam of the electric light be sent through the atmosphere in its ordinary condition, the path of the beam will be vividly revealed by the light-scattering, which is the result of its impinging upon the innumerable organic and inorganic particles of varying minuteness with which the air is charged. But if air be enclosed in a suitable chamber, freed from all currents, and left undisturbed, it is now well known that it will by the action of gravity, in time, deposit all its particles, even the most extremely minute; and the air will be left in such a condition that if the electric beam be caused to pass through it, its path—from the absence of even the most excessively minute motes—will be quite invisible. An atmosphere in this condition has been called "optically pure." Professor Tyndall has shown that in an atmosphere in this condition, properly sterilized, but putrescible fluids, remain sterile. If, however, the same fluids be put into contact with an atmosphere charged in the ordinary way with motes, they become "infallibly smitten" with putrescence. It thus appears that there is a direct relation between the condition of the air and the origin of the septic organisms which arise in sterilized fluids. The presence of the motes is, by the evidence of these experiments, essential to the production of the putrefactive organisms. The inference which comes readily and naturally to the mind is, that the germs or spore, or the bacterial equivalents of these, must be amongst the motes or particles in the charged atmosphere, and that their deposition into suitable fluids determines their development; whilst the "optically pure" atmosphere being wholly devoid of them the fluids remain sterile.

This inference is apparently irrefragable. Nevertheless there is no direct evidence, so far as the bacteria are concerned—which are the organisms specially now in question—that there are actual

* 'Nature,' Jan. 27 and Feb. 3, 1876, pp. 252 and 268; and 'Philos. Trans.' vol. clxvi. part 1, 1876.

germs; and more than this, judging by analogy it is extremely probable, from their necessary minuteness, that, presuming they exist, they are outside the limit of our present optical resources. It is therefore an actual gain to have presented to us, another method of determining the existence of particles, ultra-microscopic, as precursors to the origin of bacteria in otherwise permanently sterile fluids; because we may, at least by analogy, approximate to certainty as to whether these particles, so exquisitely minute, are or are not organic germs. If their development be carefully studied, and if we can find other organized forms nearly related to the bacteria with known and demonstrable germs, and if we can diffuse these germs through the air, and expose suitable fluids to it under proper conditions, we shall have strong evidence in favour of, or adverse to, the highly probable presumption, that the particles demonstrated by Dr. Tyndall are germs.

This idea became manifest to me immediately on studying Dr. Tyndall's facts. Now it is well known that the *Monads* or *Heteromita* are not far removed from the bacteria in structure; they are larger, and therefore more amenable to the optical resources at our disposal. During the past four or five years, by the joint labours of Dr. Drysdale and myself, it has been clearly shown that in six instances fully worked out the monads produce minute germs in enormous abundance.

Now we have shown, at first by accident, that if the residuum of a maceration or infusion be allowed to dry up, it becomes a light, hard, porous, papier-maché-like mass;* and if this be put again into a suitable fluid, the peculiar monad, which the infusion, represented by the dried mass, contained, will reappear very speedily with great vigour.

It struck me that this fact might be utilized in experiment; and I proceeded to make some investigations, which yielded most interesting results, and which were briefly recorded in a paper in the 'Popular Science Review.' †

We had proved that not only bacteria, but monads and even *Stylonichia pustulata* could live, flourish, and multiply in Cohn's nutritive fluid, ‡ which contains no albuminous matter, but only mineral salts and tartarate of ammonia. If the ingredients are all mixed the fluid becomes speedily bacterious; but if the tartarate of ammonia be kept by itself until required, and then be mixed with the salts, we have, with due precaution, an absolutely sterile but perfectly nutritive and putrescible fluid.

Passing over the experiments detailed in the 'Popular Science Review,' which my later tests have fully confirmed, the following two sets of facts appear to me to have considerable interest.

* Vide 'M. M. J.' vol. xii. pp. 262–3. † April 1876, p. 121.
‡ 'M. M. J.' vol. xiii. p. 190.

I had in my possession a maceration of haddock's head, which I had kept for fifteen months. At the time it was specially examined for experiment, it was found to contain in enormous numbers the "springing monad" * and the "calycine monad." † They were kept upon the "continuous stage" under examination for five days, and it was demonstrated that they were entering freely into the sac-condition and emitting spore. The little remaining moisture was evaporated slowly; and at length the pulpy mass was taken out and placed in the heating chamber and gently raised to 150° Fahr., which was 10° higher than was required to kill the adults. It was kept at this temperature until it was quite dry and flaky; and in some parts it was porous, cracked, and extremely friable, and under very little pressure crumbled into fine dust. A considerable quantity of this dust was obtained, and to prevent error was laid evenly on the surface of a plate of glass, and placed for ten minutes in the heating chamber at a temperature of 145° Fahr., 5° beyond the point needful to kill adult forms. This dust was now taken and diffused carefully through the air of a chamber like that used by Professor Tyndall.‡ The condensed beam of an oxyhydrogen lime-light was sent through it, and it was found that the path of the beam was far more brilliantly marked inside the chamber than in the outer air; it was therefore allowed to deposit its contained particles for four hours and a half, when the beam was found to be less brilliant but more uniform than in the air without the chamber. Ten small glass basins were now partially filled with the nutritive fluid, which was carefully mixed at the time. Six of these basins were open, and four were covered with a glass lid, which had an edge of some depth at right angles to its horizontal surface, which the more fully protected the contents from particles which merely fell perpendicularly. These vessels were inserted in the chamber, and the whole left for twenty-four hours.

On the tops of the covers of the four covered vessels were small loops for handles. In the roof of the chamber a piece of brass tubing worked into a thick piece of sheet india-rubber with glycerine, and was quite germ-tight. By a simple mechanical contrivance, a pair of light jaws could be made to open and close in this, and by means of these the looped covers on the vessels within the chamber could be taken off without interfering in the least with the chamber. This was done; § so that now all the vessels were open to the air, which by no means allowed the beam to pass

* 'M. M. J.' vol. x. p. 245. † Ibid. vol. xiii. p. 191.

‡ 'Philos. Trans.' vol. clxvi. part 1, p. 30.

§ These covers were in all cases smeared with glycerine on the outer surface, to avoid the danger in removal, which might arise from the disturbance of any germs or particles that might have been deposited upon them.

through it unseen, although its intensity was incomparably diminished.

The chamber was now entirely left for four days more, and then the first six vessels were taken out and examined. Fifteen drops were taken from each basin; five from different parts of the surface, five from the middle of the fluid, and five from near the bottom. *In every drop from every vessel the calycine monad appeared*, in full vigour, while the "springing monad" appeared in *every vessel*, but only in, on the average, ten drops out of fifteen from each. Thus the two monads contained in the infusion were developed in the Cohn's fluid from the minute particles of its solid débris scattered in the air, and made manifest by the beam.

Two days after, the other four vessels were examined in precisely the same way. But now, although thirty drops were taken out of each vessel and thoroughly searched, the calycine monad was wholly wanting in three of them; and only very feebly manifested itself in the fourth, being found only in four out of thirty of the drops taken. But the "springing monad," although not largely present, was found in each vessel, and in ninety-two out of a hundred and twenty drops.

At first this was extremely puzzling. But on reflection, it occurred to me that both the actual and the specific weight of the germs would influence the length of time taken in descending through the air, and the "calycine monad" was at once the largest form, and produced the largest germs of any of those studied. The calycine is from the 900th to the $\frac{1}{1000}$ of an inch in long diameter,* whilst the "springing monad" is only the $\frac{1}{3000}$ of an inch in length.† Here, then, was a probable explanation. The heavier germs of the larger monad had nearly all fallen before the expiration of the two days, when the covered cups were opened. The lighter germs, still lingering in the air, remained in sufficient abundance to produce the result detailed. This at least seemed an extremely probable explanation, and fortunately the means of testing it were at hand.

The smallest monad in all the series studied by my colleague and myself, was only the $\frac{1}{4000}$ to the $\frac{1}{4500}$ of an inch in length. This pours out from its sac, spore so small, as at first, to be invisible, separately, to our best powers.‡ I had an infusion some twelve months old, in which this form (the "uniflagellate monad") was very largely developed. Indeed there was nothing noticeable in it, but this form, and several kinds of bacteria with leptothrix, &c. I dried this infusion in the manner before described. The dried cake was much more homogeneous, but was still in several

* 'M. M. J.' vol. xiii. p. 191. † Ibid. vol. x. p. 245.
‡ Ibid. vol. xi. pp. 69–71.

parts easily reduced to a powdery state. It had been dried at 150° Fahr., and the dust was exposed to 145° Fahr. for fifteen minutes. This dust was now carefully mixed with some taken from the infusion in which the large calycine form had been, which I had before used. I determined to take no notice of the "springing monad" on this occasion, but confine my observations to the two contrasted forms—the minute "uniflagellate," whose spore, as I have said, at first, is invisible; and the comparatively large "calycine" form, with proportionately larger spore.

The powders were very intimately mixed, and diffused by long continued commotion of the contained air as evenly as possible through the chamber. When the larger particles had fallen, nine cups of the fluid were inserted. Only three of them were open, the remaining six having the covers on as before. At the end of twenty-four hours two of the covered vessels were exposed by the removal of the covers; and at the expiration of forty-two hours the remaining four were all uncovered. When each set had in turn been undisturbed for five days, they were subjected to careful examination. The first three yielded both forms of monad in every drop taken—the number of which was twenty from each basin; so that in sixty test-drops both the large (calycine) and the small (uniflagellate) monad appeared almost in equal proportions.

The next two cups which had been uncovered at the end of twenty-four hours, showed the small monad in *every drop;* thirty drops being taken from each cup; but the calycine or large monad, only was seen in one drop out of the whole (sixty).

The remaining four vessels were then most carefully examined, thirty drops being taken from each vessel. The result was that the small monad appeared in full vigour in *every* drop examined, and the large form *was not found at all.*

Finally, four carefully cleaned vessels were partially filled with the nutritive fluid, and put into the chamber, covered, when the last of the above series were taken out, and when the beam showed that the air in the chamber was quite moteless. The covers, however, were not withdrawn for twelve hours, that all might be as safe as possible. The open vessels were then left five days, and examined; but not a trace of a monad was seen in any drop taken from either of the vessels; and bacteria, which had been more or less present in every instance, were only sparsely, in this instance, to be seen.

The inference appears to be irresistible. The germs of minute and widely-diffused organic forms of a septic nature may be present in enormous quantities in the air, and their presence may be demonstrated, for when they are optically proved to be present, their origin being known, they are seen to germinate in suitable fluids, while the same fluids in their absence are, under the same

conditions, sterile. Moreover, difference of size affects the length of time taken by the germs in falling through the air. As all this is so entirely in accord with the facts presented by Professor Tyndall in relation to supposed bacteria germs, it appears to me to greatly enhance the probability that the supposed germs *are such*, although we have not yet discovered how they are produced, and although the microscope is unable to detect them.

IV.—*On a New Refractometer for Measuring the Refractive Index (mean rays) of Thin Plates of Glass, Lenses, Wedges, and also of Fluids placed in Cavities or Tubes.* By Dr. Royston-Pigott, M.A., F.R.S., formerly Fellow of St. Peter's College, Cambridge, &c.

(*Read before the* Royal Microscopical Society, *November* 1, 1876.)

Plate CLXII.

The ordinary refractive index is taken for white light usually denoted by the index of the line E in the solar spectrum which occupies a mean position. This line, according to Angström's recent corrections, has a wave-length of

5269·12 tenth-mètres,

which is used to denote this quantity divided by 10,000,000,000 (ten thousand millions). And I find this to be

48204·38 waves in the inch.

For general purposes the refractive index is required not for particular lines in the spectrum so much as the general effect for light in its undecomposed state. Unless the contrary is stated the index μ is the refraction for homogeneous light. If a bent oar or a raised halfcrown be observed in water, the eye has only the refraction of white light to deal with. The method here adopted refers therefore only to ordinary light considered homogeneous. At the same time it is applicable to any kind of monochromatic light.

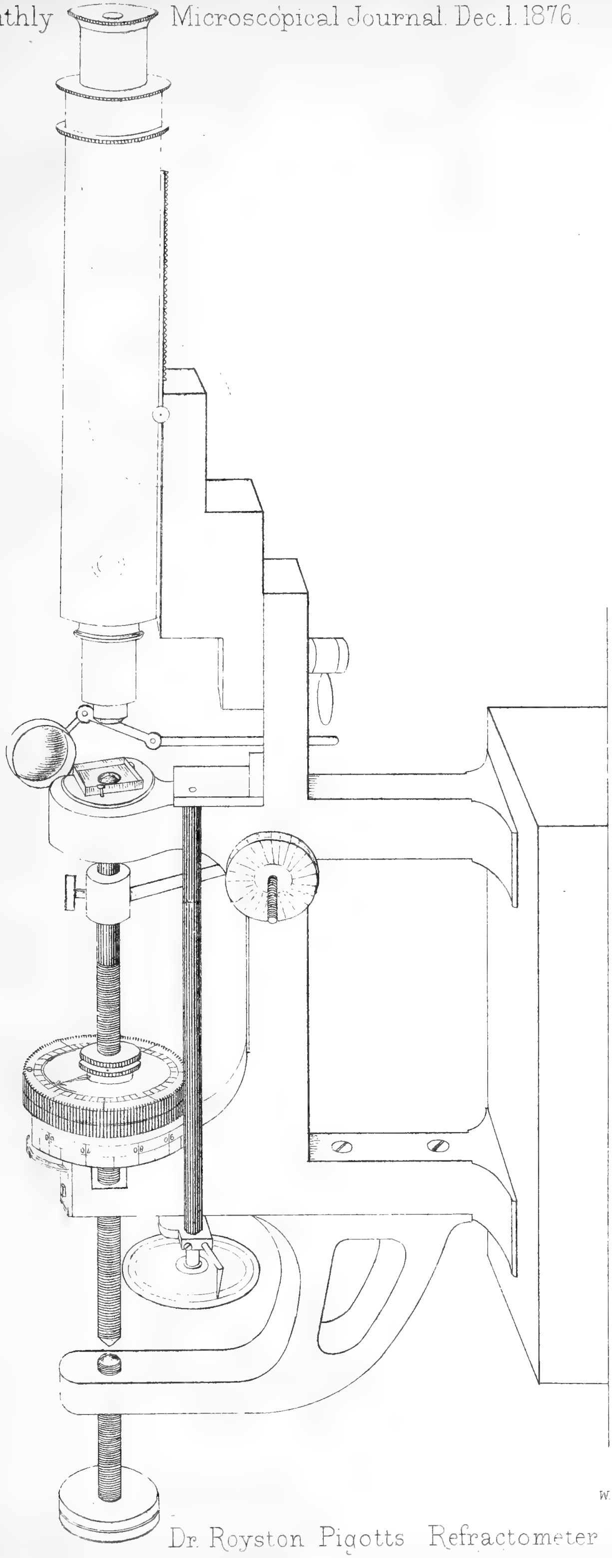
The popular aquarium presents a simple illustration of the principle employed. The bottom of the vessel appears considerably raised when viewed from above. Indeed it appears to have floated upwards nearly one-fourth of the depth. If μ be the refractive index the displacement upwards will be

$$(\mu - 1) \div \mu,$$

which in the case of water is

$$\frac{1{\cdot}336 - 1}{1{\cdot}336} = \frac{{\cdot}336}{1{\cdot}336} = \frac{1}{4} \text{ nearly.}$$

Now, if we suppose a glass tube furnished with a flat glass bottom and a flat glass cover, when the tube is filled respectively with the



W. West & Co. lith.

Dr. Royston Pigotts Refractometer

following fluids (supposing it five inches deep) the bottom as seen through the top will appear to rise upwards, as in the table:

Fluids.	Displacement by Refraction.
	in.
Spirit of turpentine	1·62
Canada balsam	1·74
Turpentine	1·76
Oil of cassia	1·94
Bisulphuret of carbon	2·02
Water	1·25

Thus in a tube five inches long turpentine raises the image of the bottom (if marked with a black cross or otherwise) half an inch higher than the water does.

It is plain, therefore, that if the filled tube be placed vertically in the axis of a long-focus microscope, capable of an adjustable position, marked by the readings of a vernier, the rise of the bottom cross can be observed.

There are two points here to be specially attended to.

1. The mark on the bottom flat should be as distinct as possible. A fine cross cut with a diamond and filled with graphite; a minute bubble in the glass, capable of illumination with a mirror, may be recommended. It is immaterial where the mark be placed, inside or outside the bottom, or indeed below it, so long as it is a fixed point. A single globule of mercury on black velvet beneath the bottom of the tube would be a charming point of reference.

2. A long-focus microscope can readily be made out of a 2-inch object-glass by shortening the tube. Suppose now that the tube be filled with water, and that

New reading (tube filled) is	1·090
Old reading (tube empty) is	2·347
Difference	1·257

In order now to obtain the refractive index we have only to subtract this difference from the length of the fluid column five inches, which gives 3·743, then dividing the length five inches by this, we get the refractive index

$$\frac{5}{3 \cdot 743} = 1 \cdot 336 \text{ nearly.}$$

Put into general language this is: subtract the displacement of refraction from the length of the column of fluid, and divide the length by this difference, the result is the refractive index.

$$= \frac{\text{Length}}{\text{Difference of length and displacement}}.$$

Example 2.

Spirit of turpentine: displacement	.. .. .. =	1·62
Distance from top	.. ..	5·00
		1·62
		3·38

$$\text{Index} = \frac{5}{3\cdot 38} = 1\cdot 48 \text{ nearly } (1\cdot 481).$$

It is well known that various readings are obtained by the spectroscope for the same substance by different instruments, according to the nature of the glass of the prisms, and on that account every observation is now referred to the standard obtained by Angström by means of the diffraction lines of Nobert.*

Thus supposing the interval between the lines B and G in the spectrum is divided into 1000 parts, the readings for E, the line for mean rays, are:

Dispersion.			Diffraction Grating.
E. 451 G. 1000	434 1000	400 1000	624 1000
Crown glass.	Flint glass.	Disulphide carbon.	

2. *Refractometer for Thin Plates and Lenses, &c.*

This instrument depends upon two principles then:

I. The distance through which an image is displaced by refraction through a plate.

II. The exquisite sensitiveness of contact-films forming the various orders of Newton's rings and the central black spot.

I. It is well known that when the index of refraction is 1·500 or $\frac{3}{2}$, an image is formed by a plate having parallel sides at a dis-

* The method here proposed is not subject to the well-known inconvenience of spectroscopic determinations, where every instrument gives different values, and eacho bservation has to be reduced to a common standard of the diffraction spectra. I find, according to my own calculation, taking the wave-length of line E, 5269·0 *tenth-mètres*, this corresponds to 48205·6 waves per inch; but it is not nearly the mean length of wave, for the mean of 9 of Angström's principal lines is

54736·9 tenth-mètres.

The brightness of the sodium lines,

D_2 43086·5 waves per inch,
D_1 43130·4 waves per inch,

naturally accounts for this mean value of E not corresponding to the mean value of the rays for white light.

tance nearer to the eye of the observer equal to two-thirds of the thickness of the plate. Indeed if t be the thickness and μ the refractive index, the displacement is generally

$$\frac{t}{\mu} \text{ (i. e. } t \text{ divided by } \mu).$$

If, therefore, an instrument could be devised which would with great accuracy measure the thickness of the refracting plate, and also at the same time the distance by which the image of points on its surface was displaced inwards by refraction, data could be obtained for determining the value of μ.

The instrument has assumed its present form after many constructions and reconstructions. I was led to consider this method of finding the refraction of glass by frequent accidents happening while using the $\frac{1}{50}$th of an inch objective with the microscope, which by pressure destroyed or cracked the thin glass covers generally applied to protect the objects or "slides." Now, so to speak, the observer always in such a case really examines the elevated image of the object, raised about two-thirds of the thickness of this cover. By knowing, therefore, the refraction of the glass cover and its thickness, such accidents, so irreparable in many valuable objects, might be avoided. Means were sought to determine the index of refraction of such covers, frequently varying from the hundredth to the thousandth of an inch thick, an extra thickness once having been destructive to a most valuable objective.

Hitherto the method of finding the refractive index has been by the use of prisms made of the material in question, and employed in the form of a spectroscope.

As an example of the power of the instrument, some flint glass, nearly half an inch thick, marked B, gave on three trials

$$\begin{aligned} \mu &= 1 \cdot 6626 \\ & 1 \cdot 6626 \\ & 1 \cdot 6621 \\ \hline \text{Mean} &= 1 \cdot 6624 \end{aligned}$$

A thin glass cover about one hundredth of an inch thick gave

$$\mu = 1 \cdot 5502.$$

The optical equation for a plate of glass,

$$v = u + \frac{t}{\mu}$$

(where u is the distance of the object and v the conjugate focus), points out that when the object is on the surface, or $u = 0$,

$$v = \frac{t}{\mu},$$

and consequently

$$\mu = \frac{t}{v}.$$

Upon constructing an instrument roughly the index was readily found to two places of decimals in covering glass, viz. $\mu = 1 \cdot 55$.

The contrivance of an instrument to read to the 100,000th of an inch now seemed desirable. It was necessary not only to measure thickness, but the elevation of the image within the substance of the plate.

The well-known delicacy of evanishment of a point under a good microscope seemed to afford an exquisite test of distance.

At first the point of an advancing screw was made to touch the "plate," the screw moving in the axis of the microscope. The point was brought into focus, or rather its image. The refracting plate was now removed, the microscope remaining undisturbed. The screw was then advanced until its point again came into focus; the point now occupied the precise position just before occupied by its image. This distance was then read off and the thickness of the plate read by observing successively the distances traversed by the screw between the point of first contact and its rising to the very same position of a particle already observed on the upper surface.

II. At this point of the research it occurred to the writer to substitute a minute plano-convex lens, fixed to the end of the screw, and endeavour to produce contact-films, especially the black central spot of Newton's rings.

In order to render the colours of these rings gorgeous by reflected light, a thin piece of plate glass was fixed at an angle of 45° behind the observing objective, and a hole perforated in the tube or "body" to admit light from a lamp; the object-glass then condensed a strong light upon the film-forming surfaces, and the black central spot came out beautifully black and distinctly defined in the field of the microscope. This occurred whether air or other fluid intervened between the lens and the surface. After fixing several different lenses, I found a radius of curvature of about one quarter of an inch the most convenient for developing the rings suitable in size for the microscopic field of view.

For being apprised of the near approach of contact, still greater convenience was accorded by using a minute film of paraffin oil, formed by repeatedly wiping the surface of the screw-lens. Contact was then heralded instantly between the two surfaces by a brilliant flash of colours. One thing appeared certain, the various colours could be produced in perpetually expanding and vanishing rings, always starting from the centre. I counted no less than thirty-two

changes of colour in the central part, reckoning from the black spot of perfect contact (half-millionth of an inch thick) and the final evanescence of the last colour by slight movements of the screw.

For the purpose of illuminating the point of contact of the "screw-lens" with the plate under examination, I inserted a minute right-angled prism behind the lens. This giving totally reflected light, provided nothing adhered to the reflecting surface of the prism, afforded the means of making observations by ordinary daylight, and observing the rings of Newton, though very pale, by transmitted light.

Another great advantage of the prism-lens attached to the end of the steel measuring screw, is the bright illumination of the contact surfaces, the contact of an opaque extremity of the screw being with difficulty ascertained.

The whole method of finding the refractive index of a thin plate of a given refractive material resolved itself, then, into the best instrumental means for advancing a fiducial visible and illuminated point truly and steadily through measurable intervals, and observing with a good microscope the precise position of evanishment in and out of focus, and determining the focal points under correct collimations.

After many trials the following form was adopted:

A steel cylinder very accurately turned between *dead* centres (i. e. the centres being fixed and the object revolving between them), about 5 inches long and $\frac{3}{10}$ inch in diameter; upon this screw-threads (very nearly 101·3 per inch) were very patiently formed. The front part of the cylinder passes smoothly (at first air-tight like a piston) through a collar of brass, into which it had been very slowly and carefully ground (with the finest cutting powder and oil); at about two inches of the other end was formed a screw as described, with a very exact apparatus lent to the writer by a celebrated optician.

It was found that in so delicate an operation as dealing with coloured films, touching any part of the instrument caused them to flash a new colour. It was necessary to obviate, then, all varying mechanical strain. Springs so common in micrometers to obviate "loss of time" were found to introduce, from their varying pressure, very variable errors: after a time I was compelled to abandon their use altogether.

The constant force of gravitation and dead unvarying weight was now introduced; and in order to compensate possible deviations in the true spiral form of individual threads (every one of which was carefully examined with a strong magnifying power), a nut was formed, so as constantly to embrace (unlike ordinary micrometers of the usual form) precisely the same number of threads in

every measurement, so that on the average the same number of threads probably represented nearly the same distance. In the case where a spring is more and more compressed and the number of threads embraced by the nut is constantly increasing (although some compensating action may arise), the screw and parts are certainly submitted to very varying and uncertain conditions. Another source of error arose—*shake of rotation.* Having abandoned the usual form of micrometer construction (a revolving nut with a constant change in the number of threads embraced—a plan, one would think, fatal to all delicate accuracy), the next difficulty was to ensure to the steel screw absolute advance and retreat without *rotatory* shake or motion. For this purpose *slides* were also abandoned. This action, the most important part of the instrument, should now be described.

A lever is affixed to the cylinder of steel and bent at right angles; it carries an adjustable weight. This weight slides upon a flat edge formed parallel to the axis of the steel screw by a most careful process, tested by a carefully prepared spirit-level for parallelism with the axis of the screw.

On lifting the weight slightly, the lever rotates the screw through a small angle; and this lever forms a constant test of the efficiency of the screw action of the greatest sensitiveness.

A further action put into motion by a fine screw gives to the advance of the film-forming surface, or prism-lens, a movement of the millionth of an inch.

Supposing that the recording wheels have advanced several turns, the weight and lever also advance on the smooth edge already said to be formed parallel with the axis of the screw.

The constancy of the weight preserves the screw in one normal fiducial position, as regards its liability to rotate on its axis. An error of one hundredth of an inch in the sliding edge would produce an error of the reading of less amount than the hundred-thousandth of an inch.* But the lever advances so very slowly, as the wheels rotate the nut upon the screw, that this error appears to be almost destroyed.

An arrow-head shows upon the face of the differential wheels the number of turns taken by the nut. The instrument is self-recording, and reads to four places of decimals, from the hundredth of an inch to the hundred-thousandth. Two wheels, divided into 98 and 100 teeth respectively, run in gear *at will* with a long pinion of ten leaves, carrying a wheel showing the hundred-thousandths of an inch.

The prism end of the screw passes through the ground socket;

* The path of the weight on the lever would be for a complete rotation about 20 inches; the proportion of $\frac{1}{100}$ to this is 2000, and the two-thousandth part of a revolution is two-thousandth of $\frac{1}{100} = \frac{1}{200000}$.

and this socket carries a small stage accurately turned and ground, furnished with stops and a spring to confine the small plate of glass, if necessary, in one position. The axis of the screw and of the observing-microscope are carefully adjusted in one line, so as to have a common collimation.

In very thin glass an objective of quarter-inch focal length has been found sufficient. For thick glass, nearly half an inch thick, Mr. Wray, of Highgate, made a beautiful half-inch with three lenses cemented together with balsam, so as to give the greatest possible penetration. The body of the microscope is about six inches long.

The whole instrument is placed at an angle of about 40° upon a mahogany frame. Its accuracy depends on the weight of the toothed wheels always bearing with an equal pressure, without springs, on the back poppet of the jeweller's lathe employed to carry and work the apparatus.

It remains to say a few words on the method of using the instrument.

1. *By transmitted Light.*—The instrument is placed near a window in daylight, and the small condenser is then used to throw a light upon the minute prism, which is then reflected up the microscope. A minute drop of kerosine is placed on the prism-lens at the end of the micrometer-screw, and then wiped off.* The lens is now withdrawn a little below the stage, and the plate of glass to be measured is placed upon it. The microscope is armed with a quarter-inch, a half-inch, or inch objective, according to the thickness of the plate to be examined.

The microscope is then focussed upon the under surface of the plate. The micrometer-wheels are set in motion. The prism-lens gradually rises in view. The instant of contact is observed by a sudden spreading out of the remains of the oil-drop. It may require several cleansings or wipings of the lens before the oil is sufficiently removed. The film expands and contracts with the slightest movement; with a little practice the eye detects the position of initial contact.

The instrument is then read.

Example.—Initial reading for a piece of "covering glass" one hundredth of an inch thick:

$$I = 0 \cdot 0044.$$

The glass was removed and the prism-lens advanced until its surface just came into focus. The distance was then read.

$$\begin{aligned} D &= 0 \cdot 0086 \\ I &= 0 \cdot 0044 \text{ initial reading.} \\ \hline \Delta &= 0 \cdot 0042 \text{ the elevation of image.} \end{aligned}$$

* Many wipings appear still to leave a minute quantity which the experiment really requires to be left to form a visible film.

Replacing the glass and again repeating the process the initial reading at the lower contact was now found not to be 0·0044, but

$$I_2 = 0\cdot0045 \text{ (an extra } \tfrac{1}{100000} \text{ inch).}$$

Viewing an exceedingly minute scratch on the upper surface by re-focussing upon it, the reading for thickness was

$$\begin{aligned} T &= 0\cdot0163 \\ \text{Initial reading} &= 0\cdot0045 \\ \hline \text{Thickness} &= 0\cdot0118 \\ \Delta &= 0\cdot0042 \text{ elevation of image.} \\ \hline v &= 0\cdot0076 \text{ distance of image from upper surface.} \end{aligned}$$

$$\text{Therefore} \quad \mu = \frac{t}{v} = \frac{0\cdot0118}{0\cdot0076} = 1\cdot55 \text{ nearly.}$$

I obtained from Mr. Browning several square pieces of flint glass of varying density and colour, and found by a series of measurements:

		Value of μ for mean rays.
A.	Clear white flint	1·537
B.	Yellow and heavy	1·6626
C.	Yellowish, and very heavy	1·723
D.	Strong yellow, and the heaviest of them all ..	1·7555

2. *By reflected Light.*—Far more interesting are the phenomena developed by reflected light, throwing the light laterally upon the inclined transparent plane within the microscope, the object-glass of which acts as its own condenser. The most beautiful colours are developed on contact, either with a film of air or kerosine.

The central spot of final contact is of a grey-black, surrounded with its well-known succession of Newtonian rings of great beauty and perfection, flashing through numerous changes of colour for each thickness of film varied by the micrometer-screw.

The instant of the formation of the black spot film (thickness 0·000005 inch) determines the place of contact, i. e. the fiducial point of zero, with a precision scarcely equalled by any other known method of linear measurement by optical means. It can be readily obtained with great accuracy. This important point (the zero) of the observations depends not upon a thin spider line or engraved line, but upon the formation of a bold black circular spot, whose diameter is variable and dependent upon the curvature of the lens employed. Where great endurance is desired, a small plano-convex sapphire lens can be cemented to the prism at little expense.

The behaviour of minute microscopic kerosine oil-drops per-

sistently adhering to the surface of the lens, in spite of repeated wipings, is worthy of notice.

On the near approach of the lens to the under surface of the glass under notice, the scattered drops suddenly coalesce, shooting out into a film of varying colour.

On one occasion a small oil-drop, one hundredth of an inch in diameter, appeared as a black annulus enclosing a bright thin ring of light, which enlarged on being touched by the prism-lens by the advance of the screw. It spontaneously then spread out and rapidly exhibited within its centre a sudden display of very minute but richly coloured Newtonian rings, formed in this case by interior reflected light, although transmitted light was then being employed.

It is not absolutely necessary that a plate of glass with precisely parallel sides be used. A wedge can be manipulated if a particular spot be chosen and the wedge be most carefully adjusted to the same position by means of the stop and ledge on the stage. Less difficult, however, is glass formed into a plano-convex lens of long focus, the plane side being placed downwards, and the same point, the summit, if possible, being always selected for observation; better still if a slice be cut off so as to present a secure fixing of the lens in the same position.

A variety of substances formed into plates, wedges, or lenses, with little convexity, may thus be examined, as also fluids enclosed between parallel plates, as well as enclosed in a tube, as already described; or by the instrument exhibited under the condition that the fluid be enclosed in a thin glass cell, which is completely to be filled with the given fluid.

The instrument is by no means intended to compete with the spectroscope. But it gives the mean refraction in a variety of cases where that instrument is not applicable.

V.—*The Gladiolus Disease.*

By Worthington G. Smith, F.L.S.

Plates CLXIII. and CXLIV..

For many years past the Gladiolus has been subject to a damaging and singular disease. As in many other diseases of plants, all sorts of conflicting opinions have been expressed regarding the *nature* of the disease—some growers almost denying the existence of any disease whatever, whilst others have described it as so bad as to threaten the almost total extinction of the Gladiolus as a garden plant in this country. As in the case of the murrain of potatoes, peach blister, &c., different observers have had different conditions of the host plant in view; some writers have attributed the disease to a fungus, whilst others have totally denied the presence of any fungus whatever. Amongst all these conflicting opinions the fact remains that there is a Gladiolus disease, and one singular in its nature, for the cause is at present imperfectly understood.

As far as my experience goes the Gladiolus disease is invariably most virulent in damp, heavy soils, and in wet seasons; in well-drained, dry soils the disease is almost unknown. It is much more destructive in England than in France, simply because the latter country has a clearer and less humid atmosphere. Just as in my experience of the potato murrain I have found the first attack to be almost invariably made upon the seed tuber whilst in the ground, so I have observed in the Gladiolus the first part attacked is almost invariably the seed-corm which is planted, though the attack may be made *before* as well as *after* planting. When growth commences the diseased condition of the seed-corm rapidly spreads to the sprouting leaves and petioles, and the plant of the year is destroyed. It does not follow as a consequence that the new offsets must be diseased, for the offsets from a diseased corm are frequently quite sound, though it is possible they may have the germs of disease in their constitution, which will only show themselves in a bad form in the spring which follows. It is exactly the same in the potato disease. Under certain conditions of dryness, diseased seed potatoes will produce healthy plants and tubers free from the murrain. When the corm of the Gladiolus is badly diseased it is shrivelled, and permeated throughout with a rich red-brown colour. When the corms are lifted from a damp soil they are infested with the spawn of different fungi, and as decomposition goes on the corms are at length totally destroyed by diverse fungi, infusoria, nematoid worms and mites.

I have often examined the diseased corms of Gladioli, and made notes of the various parasitic fungi found in and upon them, but

till lately nothing has struck me as being especially new or different from what one might expect to find upon decaying bulbs or corms of any variety.

There is, however, a puzzling and singular *mycelial* growth, commonly found upon diseased Gladiolus corms, which has been pointed to with good reason as the probable cause of the disease. This mycelium is not peculiar to the Gladiolus, for the same pest destroys the bulbs of *Crocus sativus*, the bulbs of Narcissus, and attacks potatoes, asparagus, and other plants. It was described long ago by Dr. Montagne, and is known in France under the name of *Tacon*, and in this country as "copper-web," or *Rhizoctonia crocorum*, D.C. This "copper-web" is obviously very imperfectly understood, for at present the fruit is unknown: in fact, the very name of Rhizoctonia (like Rhizomorpha) has almost fallen out of use.

In March of the present year the Rev. H. H. Dombrain furnished me with a Gladiolus corm in a very bad state of disease. It presented the usual appearances of the Gladiolus disease as just described, and was a mature seed-corm destined to bloom this year, and not a young offset. On minutely examining this corm under the microscope I found all the cells and starch destroyed, probably from the previous presence of some corrosive mycelium, and the whole interior more or less filled with the bodies here illustrated (Plate CLXIII.). Whether these bodies are in any way connected with the threads described under Rhizoctonia there is no evidence to show, for in the first instance we get threads without fruit, and in the new instance now brought forward, fruit without threads, but both the threads and fruit apparently produce the same *effect of disease* upon the corm. Further investigation must clear up this point, but in the meanwhile the bodies detected by me are *undoubtedly new.*

Attention may here be called to the large and magnificent crystals so abundant in Gladiolus corms, and shown in this illustration. Crystals are always formed in cells, but here the great crystals are many times larger than the largest of the decomposed cells of the corm. This phenomenon can only be explained by the probable fact of the crystals aggregating and recrystallizing after the cells have been destroyed by the corrosive mycelium.

Different views have been expressed as to the nature of the compound spores found this year in Gladiolus corms by me. At first sight they appear to superficially resemble the resting spores of a Peronospora, but this view may be at once dismissed. They greatly resemble Papulaspora, but I am convinced by several characters that they do not belong to this genus, or indeed to any mould, but to the order Cæomacei.

These compound spores bear a strong resemblance to the spores

of Thecaphora, but I believe they really belong to a new species coming under the genus (closely allied to Thecaphora), named Urocystis. This new species I propose naming *Urocystis gladioli*, and it may be characterized as follows: Sori (or clusters of spores in blisters) obliterated or effused, spores large, compound, consisting of from three to six inner brown cells, and a larger indefinite number of nearly transparent outer cells, both series of cells being fertile. Habitat—On and in the corms and scapes of Gladioli. (See Plate CLXIII., enlarged 200 diam., and A, B, Plate CLXIV., enlarged 1000 diam.)

Both the brown and white cells burst, and throw out threads of mycelium. Further observations can alone show whether this mycelium, under certain conditions, may now be capable of existing on diverse hosts as mycelium only, and so put on the characters of Rhizoctonia.

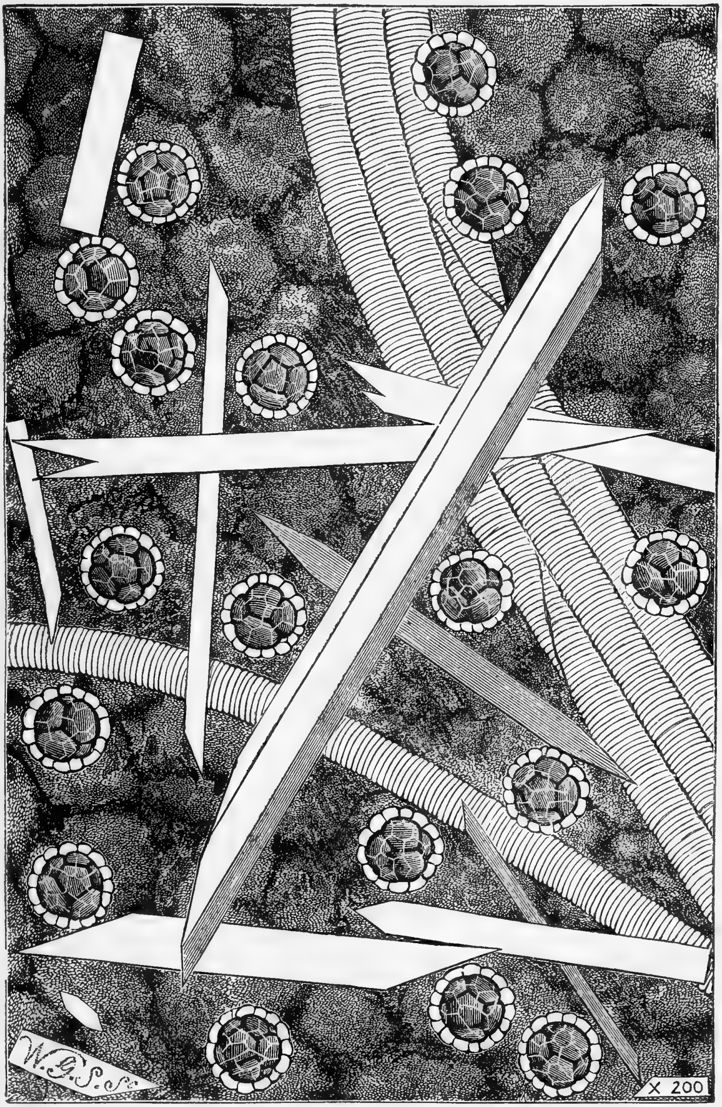
The effused or obliterated sori, or spore blisters, point rather to the genus Thecaphora than Urocystis, but I consider the salient characters belong to the latter genus, and make it the proper one for the reception of the fungus under consideration. Dr. Wittmack, Dr. Magnus, and Dr. Brefeld, of Berlin, have examined my preparations, and they consider the compound spores to belong to Urocystis. As to the peculiar habit and the obliteration of the sori, Dr. Brefeld says he has seen Urocystis growing on very different materials, even upon bread. Dr. Brefeld considers Urocystis to be a Sclerotium, or a compact spawn or mycelium in a state of rest, but this is not my view.

Whilst describing this curious fungus it may be well to pass briefly in review its immediate allies as found in this country, with illustrations taken direct from nature. This will at once show the strong family likeness between the new *U. gladioli* and its neighbours. I may say at this point that Urocystis is sometimes described under Polycystis.

The first is *Thecaphora hyalina*, Fing. (C, D). This species, the only one of its genus, is closely allied in habit with *Urocystis gladioli*, for it is without sori; the compound spores are, however, in one series—not two, brown and large, and transparent and small, as in Urocystis. The habit of this Thecaphora is most peculiar, for the fungus grows inside the seeds and seed-capsules of different species of convolvulus. There is no external evidence on the seed-capsules of the presence of the fungus within.

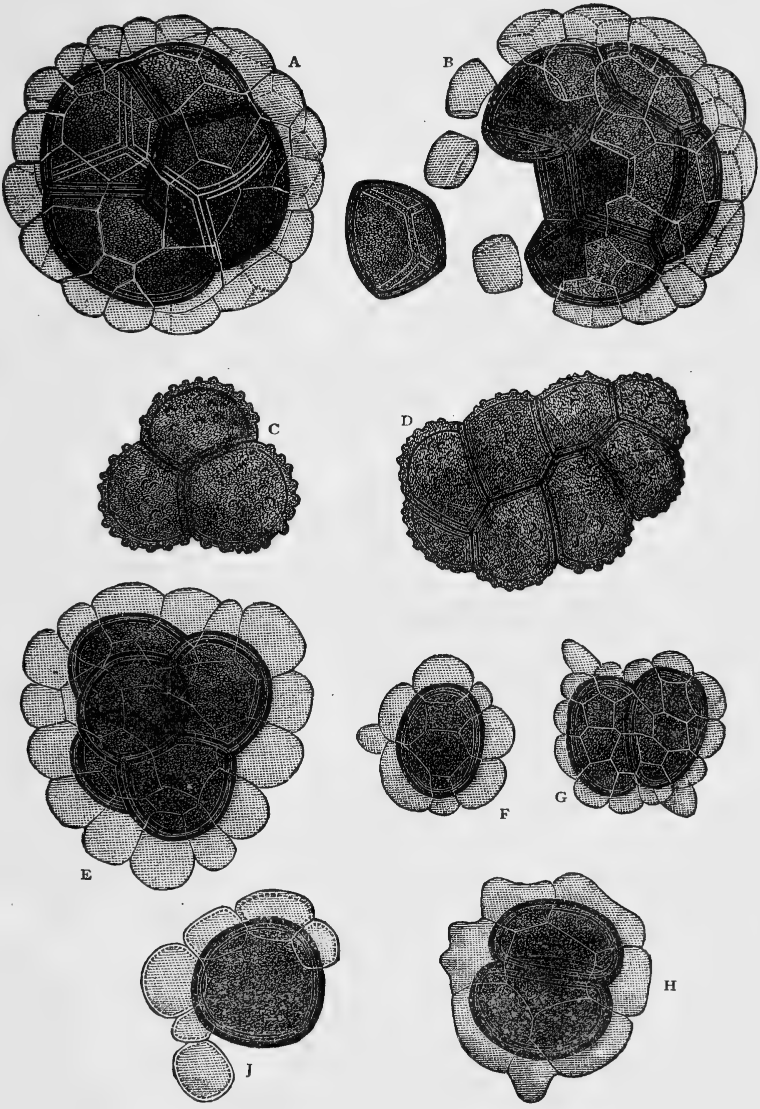
Urocystis violæ, B. and Br. (E), is a common plague of violets; it causes large gouty swellings upon the petioles and leaves, and otherwise deforms the entire plant. At length the swellings burst, and discharge the innumerable spores.

Urocystis colchici, Tul. (F, G), a similar plague with the last. It grows upon colchicum, but is less apparent in its effects.



GLADIOLUS DISEASE.

Fragment of Diseased Corm, showing decomposed cells, spiral vessels, crystals, and compound fungus spores, *Urocystis gladioli*. (Enlarged 200 diameters.)



COMPOUND SPORES OF UROCYSTIS AND THECAPHORA.

A, B, *Urocystis gladioli* (at B the compound spore is shown in the act of breaking up); C, D, *Thecaphora hyalina*; E, *Urocystis violæ*; F, G, *U. colchici*; H, *U. occulta*; J, *U. pompholygodes*. (Enlarged 1000 diameters.)

Urocystis occulta, Preuss (H), a pest found on rye and various sedges.

Urocystis pompholygodes, Schlecht (J)—a disease of the Ranunculaceæ; like *U. violæ* it causes great distortion of the host plant, and makes large gouty swellings, which at length burst and discharge an immense number of spores.

Most growers of Gladioli will probably be dissatisfied with this paper and its hard names, but it must be remembered that it is an attempt at a diagnosis of the Gladiolus disease, and treats only of the characteristic and distinguishing symptoms; for, till a disease is understood, a remedy is always out of the question.

Cure is probably quite within reach ; but the discussion of this subject must be reserved for another time.—*Gardeners' Chronicle*, pp. 420, 421, 422: 1876.

PROGRESS OF MICROSCOPICAL SCIENCE.

The 'Challenger' Report on the Globigerinæ-ooze.—In a recent number of the Royal Society 'Proceedings' (No. 170), there has been a Report of the 'Challenger' expedition, which fills the entire number. In this, among other things, the following remarks occur in respect of the *Globigerina*-ooze. The reporter, Mr. J. Murray, says that after the deep-sea clays this is the most abundant deep-sea deposit. It has occurred at all depths from 250 fathoms to 2900 fathoms. The *Globigerinæ*, which give at once the name and the chief characteristic to this deposit, are really found all over the bottom of the ocean. Even in our deepest clays, if the surface layers be selected and all the amorphous matter be washed away, one or two shells of some variety of pelagic Foraminifera can usually be detected. By pursuing this method I have only failed on one or two occasions. They appear to be quite absent in the Arafura Sea. It is, however, when they occur in vast numbers that they form the deposit known by this name; at least such is the sense in which it is here used.

We did not find a *Globigerina*-ooze in any of the enclosed seas, in the Southern Ocean south of lat. 50° S., nor in the North Pacific north of lat. 10° N.

In the Southern Ocean only one small species of *Globigerina* was found in the surface waters; but in the North Pacific many varieties of pelagic Foraminifera abound near the surface of the ocean.

In other parts of the preceding oceans, and in the other oceans we have visited, it occurs in irregular patches, being always present in the ocean when we have depths of less than 1800 fathoms. Its presence or absence at depths beyond 1800 fathoms is, however, determined by conditions at present unknown. A number of varieties occur both as to colour and composition. Some specimens are nearly pure white, others have a rose colour, and others are red or dark brown. The red and brown colour arises from the presence of the oxides of iron and manganese. In the white varieties the sediment, after dissolving away the carbonate of lime, is in some specimens abundant, in others not abundant, and is either of a red or slate-blue colour. We find the former colour to prevail in those soundings far from continents and large islands, and the sediment is not abundant except where pumice or scoria is present. The latter, or slate-blue colour, is found in those soundings more or less near continents and large islands; and it is suspected that this sediment has its source chiefly from the disintegration of these adjacent lands.

Mica, quartz, pumice, scoria, and other mineral particles are met with; but in those soundings farthest from land a little piece of pumice or scoria may be the only trace of mineral particles.

In some specimens there are very many remains of organisms with silicious shells, as Radiolaria, Diatoms, and Challengerias; but in others these remains are almost entirely wanting. In three soundings in mid-Atlantic between the Canary and Virgin Islands, and in

several soundings in the South Pacific, manganese in the forms of grains and nodular concretions is very abundant. As a rule, however, this substance occurs rather sparingly in *Globigerina*-ooze. In some instances we get little nodules of these bottoms, the shells as it were being run together by a silicious cement. Many small pieces of cherty-like mineral also occur, which are angular and soft, and do not look as if they had been transported. Manganese nodules occurring in the *Globigerina*-ooze have often a nucleus of a yellow and green colour, in which *Globigerina*-shells can be seen; but their carbonate of lime has been entirely removed, and replaced by a silicate. There are reasons for thinking that these indications of flint (?) occur only in those samples where the silicious shells of Radiolaria, Diatoms, &c., are wanting, and do not occur where these organisms are present. A re-examination of all the bottoms must be made before this statement can be definitely affirmed. Casts of Foraminifera occur very sparingly in *Globigerina*-ooze; in the purest samples not at all. In those with an admixture of clayey matter we have frequently one or two partial casts of a very rough character. In two soundings, Nos. 211 and 301, in the Pacific, we found the Foraminifera not only filled, but also coated with a red substance, so that we had both an internal and an external cast, the two being connected by little rods representing the foramina of the shell. In these soundings there was much clayey matter and disintegrating pumice and scoria.

In a few soundings in the Pacific, as No. 304, we have had a *Globigerina*-ooze on the surface of the bottom, and a foot beneath a nearly pure red or brown clay. Again, as in Nos. 268 and 307, we have the reverse arrangement, a clay occupying the surface, and the deeper layers having many *Globigerinæ*. In all these cases the surface layer has been normal with the other soundings in the same region as to depth. In the first case we might bring in elevation to account for the *Globerina*-ooze overlying the red clay, or we might suppose that chemical changes are going on in the deeper layers which remove the carbonate of lime. In the second case we may account for a red clay overlying a deposit with many *Globerinæ* in it by supposing a depression of the bottom after the latter had been laid down; or we may believe that agencies are now removing carbonate of lime from the surface layer, and that these were not active in some past time.

This deposit occurs, in one sounding, in the Pacific at a depth of 2925 fathoms in mid-ocean. In the eastern part of the Atlantic it occurs also at great depths.

Professor Nordenskiöld's Microscopic Examination of Volcanic Dust.—Professor Nordenskiöld, in a paper which has been translated by our contemporary, the 'Geological Magazine,' gives the following account of the dust which was brought down by a snow-storm on the hot-beds of one of the Royal Swedish residences. He states that "some of the dust was collected and examined under the microscope, and found to consist for the greater part of small, translucent (or transparent), angular, uncoloured, glass-like particles, which formed elongated filaments, bent sabre-fashion, or sharp-cornered flat bodies,

partly plain, partly connected together in the form of Y or T. The filaments are commonly full of cavities, or pierced in the direction of their length by hollow canals, whereby they are often light enough to be able to float on water. On being examined in polarized light, most of the grains of dust are found to be isotropic—that is, without action on the polarization plane of the light passing through them. Only exceptionally can there be discovered under the microscope doubly refracting crystalline particles, presumably of augite or felspar, and non-transparent black grains of magnetic iron-ore, that may be drawn out with the magnet. No traces of metallic particles could be discovered in the dust by trituration in an agate mortar and washing, nor did chemical reagents show the presence of cobalt or nickel." He gives the following explanation of its origin: "Under the microscope the Haga dust has in many respects a striking likeness to the finest dust from a so-called 'Bologna drop' that has sprung asunder, that is, a drop of glass which has been cooled suddenly, and therefore, from the most inconsiderable cause, for example, a scratch with a file on its surface, falls asunder to a fine powder. Here we have possibly a hint as to the formation of this dust. On the outbreak of the volcano, an immense quantity of superheated steam and strongly compressed gases has violently escaped out of the crater's lava-sea, and brought along with it masses of its glowing contents more or less finely divided. Naturally the particles of lava, which at first are in a molten state, not only solidify suddenly, but are also cooled to a very low temperature in the upper strata of the atmosphere, and thereby obtain the property of the 'Bologna drop,' of springing asunder, with the least concussion or shaking, to a fine dust."

The Kentucky Meat Shower. — This strange phenomenon, which produced a large amount of scepticism on the part of some, has been now very thoroughly explained in the following letter of Dr. Mead Edwards, in a late number of the 'Scientific American.' After describing the occurrence at some length, he then states the results of his examination of portions of the so-called shower which fell at Kentucky, and says:—"Being determined to follow the matter up, I wrote to Mr. Parker, and he very kindly sent me three specimens, two in the natural state as they fell, and one prepared and mounted for the microscope. The last named consists entirely of cartilage; one of the others is likewise a mass of cartilage, while the remaining portion shows a few striated muscular fibres, along with what appears to be dense connective tissue, but in such a condition that its exact character cannot be well made out. I am promised further specimens and information by Mr. Parker, who has been unsparing in his endeavours to elucidate the mystery; whilst he has been, at the same time, extremely liberal in the matter of distributing specimens. So much for the facts. Every specimen I have examined has proved to be of animal origin, showing that the Kentucky shower was a veritable 'meat' shower. As to whence it came I have no theory. Mr. Parker informs me that the favourite theory in the locality is, that *it proceeded from a flock of buzzards, who, as is their custom, seeing one of their companions disgorge himself, immediately followed suit.* In

fact, such an occurrence has been actually seen to occur, so that it would seem that the whole matter is capable of a reasonable and simple explanation, and we may expect to hear of similar downfalls in other localities."

The Structure of the Cells of the Spinal Ganglia.—This, which has many times been investigated, has lately been taken up by Herr R. Arndt, who, in a paper published in Max Schultze's 'Archiv' (vol. xi.), states that the typical form of the cells of the spinal ganglia is a more or less irregular flat disk. They are at least bipolar. The author believes that multipolar cells also exist, which, in addition to two strong and well-marked processes, send out a number of finer ones, which are, however, easily torn off or overlooked. Herr Arndt was unable to convince himself of the existence of unipolar ganglionic cells. The apolar bodies which do occur in the spinal ganglia, the author regards as the result of an anomalous development. The two chief processes of the ganglionic cells generally arise very near each other. In many cases each process arises from the ganglionic cell by itself, and is enclosed in a special sheath, which is a continuation of the capsule. In other cases the two processes approach each other, and are enclosed in the same sheath. They are medullated almost at their origin, but the author has observed processes originally non-medullated.

The Vocal Organs of the Cicada.—The 'Academy' has recently published among its interesting microscopical notes an account of a French investigation of the above-named organs. It says that M. Carlet, who has been inquiring into the subject, finds that the researches of Réaumur, Carus, Duges, and others, are somewhat inaccurate. M. Carlet (in the 'Comptes Rendus') states that the singing cicada has three pairs of thoracic stigmata, the two first situated immediately below the spiny plates of the mesothorax, while the two last belong to the metathorax, and are covered by the spiny plates of this thoracic segment, which are the lids of the musical apparatus of the male. These three pairs of thoracic stigmata "he is certain exist also in the females." The two last thoracic stigmata, he states, were mistaken by Carus for the two first of the abdomen, from which they differ in position and structure. The thoracic stigmata are very large, hairy at the margin, surrounded by a horny circle, and furnished with movable lids, while the abdominal ones are small, punctiform, destitute of movable lids, and surrounded by a little mealy aureole. There are seven pairs of abdominal stigmata, not six, and the first are situated on the scaly triangle of the first abdominal ring. The second pair have no mealy circle, and are less visible than the others. The external wall of the sonorous cavity in which the drum is situated does not, according to M. Carlet, belong to the first abdominal ring as Réaumur figured it, but to the second, as is readily seen in *C. orni* and *C. maculata*, in which the wall is incomplete, and forms a salient apophyse projecting on the second segment. In *C. plebeia* it is easy to see that the superior margin of this external wall is free, and separated from the upper margin of the

cell of the drum, which is formed by the first abdominal ring. Previous observers have described an extensor muscle of the drum; but the muscle has another function, as the author proposes to show in another paper.

How to arrange Diatoms.—Among the many methods that have been devised for this purpose is one recently described in the 'American Naturalist' by Dr. G. C. Morris, of Philadelphia, who, that journal says, arranges diatoms with facility and success, by using the mechanical stage as a means of holding and moving the bristle which handles the diatoms, while the sub-stage prolonged upwards (through the opening of the regular stage) by means of a tube, serves as a stage to hold the object slide. An arm, attached by means of a socket to the stage, carries a small cork, through which is passed a needle, and the bristle is fastened to this needle in such a manner as to project about a quarter of an inch beyond its point. With this arrangement the objective can be readily focussed upon the bristle-point, which can then be moved in any horizontal direction, while the object can be brought up to focus, or depressed below it, by means of the rack of the sub-stage.

Contents of Foreign Journals.

It would be of course impossible to give abstracts of even one-tenth part of the total number of scientific journals that are published on the Continent, and that relate either fully or in part to microscopic labours. But at the same time we consider it most useful to give the contents of these journals, for then the worker will be enabled to see at a glance where the particular information he requires is to be had, and a visit to either the libraries of the Royal or the Linnean Society will at once place him in possession of the entire paper and illustrations upon the question in which he is interested. In fact, it will be to the independent researcher more valuable information than an abstract, which he would not probably rely on.

Nederländisches Archiv für Zoologie. Edited by C. K. Hoffmann. Band 3rd, Heft 1st.—This contains a valuable paper on the Structure of the Retina in Amphibia and Reptiles, by the Editor, with two excellent plates.—The Development of the Entomostraca, by P. P. C. Hoek, with two most excellent tinted plates, certainly vastly superior to anything we could attempt in this country, though the journal is published at Leiden.—And, lastly, on the Structure of the Synovial Membrane, by J. G. Van der Sluijs, which is also illustrated by a plate.

Zeitschrift für Parasitenkunde. Edited by Dr. E. Hallier, of Jena. 4th Band, 2nd Heft.—This has only one microscopical paper, by the Editor, "On the Results of Microscopic Research." It has to do, of course, with the subject on which the journal deals.

Zeitschrift für Wissenschaftliche Zoologie, vol. xxvi. part 2.—On the Chilostomous Bryozoa, by W. Repiachoff. — On the Gastrotrichous Rotifers, by Dr. H. Ludwig.—The Anatomy of *Chætoderma nitidulum* (a sipunculous echinoderm), by Herr L. Graff.

Reichert and Du Bois-Reymond's Archives, part 1, 1876.—On the Anatomy of the Leaf of *Dionœa muscipula*, by Herr F. Kurtz. 2 plates. —Part 2. The Allantois in the Embryo of Man, by Professor W. Krause.

Nuovo Giornale Botanico Italiano.—(The contents of these are thus given by 'Nature,' July 6.) The two last numbers are chiefly occupied with Italian botany, viz. on the Structure of the Wood of *Periploca grœca*, by Signor A. Mori, and other papers, hardly of interest to the microscopist.

Archiv für Naturgeschichte. Edited by Dr. Troschel. Forty-second year. 1st Heft.—An interesting paper, accompanied by two good plates, on Helminthological Studies, by Dr. von Linstou.—On the Development of the Mites, by Dr. P. Kramer. This has one plate connected with it.—On the Natural History of a species of the family *Gamasidiœ;* also with a plate, and by the last-named author.

Annals des Sciences Naturelles. 6th series.—Botany: Development of *Scleroderma verrucosum*, by M. N. Sorokine. This is illustrated by a pair of excellent plates.—The same author has written the following papers: "A New Genus of *Myxomycetes*," and also "A Few Words on the Development of *Aphanomyces stellatus*."—The last paper in this number which is of interest to the microscopist is on the reproduction of Ascomycetes, a microscopical and morphological study, by M. Max Cornu.

NOTES AND MEMORANDA.

The Fluorescent Ray for Microscopic Purposes.—At a late meeting of the Academy of Natural Sciences of Philadelphia, some remarks on which we shall not comment were made by one of the academicians. They are thus reported:—Professor Frazer spoke of thinness or minuteness of objects under the microscope, and suggested a method of studying, by means of the fluorescent ray, objects at present invisible to the highest powers. Dr. Hunt stated in reply that microscopists were not willing to be limited in their observations by the calculations of mathematicians, and that the comparative darkness of the fluorescent ray would not be favourable to investigations of the kind.

Effect of Aperture on Definition.—Mr. J. Zentmayer, in a very clear lecture on the elementary properties of lenses, published in the 'Journal of the Franklin Institute,' May and June, 1876, and which we should reprint were it not for the number of illustrations it would require, calls attention prominently to the confusion of images necessarily attendant upon large apertures, except when viewing absolutely flat objects, from the stereoscopic character of the images formed by different portions of the surface of the lens, the image formed by pencils transmitted by one side of the lens being unavoidably different from corresponding images formed by the opposite side of the lens.

Examination of Wool by the Microscope.—A valuable report has been published in the 'Bulletin of the National Association of Wool Manufacturers' (an American body), which is by Dr. J. J. Woodward, the distinguished microscopist, and Dr. J. L. Leconte. The 'American Naturalist' furnishes us with the following account of the results arrived at by these two gentlemen:—The kinds of hairs observed and described by the commission may be conveniently arranged in three groups. First, woolly hairs. These mostly extend "from half an inch to several inches in length, without any medulla, and without perceptible taper. They present (especially in the wool of the sheep), at frequent but irregular intervals, well-marked, one-sided, more or less spirally arranged thickenings of the cortical substance, which gives to the wool its curly character. The mean diameter of each hair varies from $\frac{1}{500}$ to the $\frac{1}{1000}$ of an inch, or even less; and the scales of the cuticle are so arranged that their free edges project somewhat, forming well-marked imbrications, of which usually from fifteen to thirty can be counted in the $\frac{1}{100}$ of an inch." Such hairs constitute the wool of commerce, originally limited to the sheep but now applied to the goat, camel, and llama, and similar hairs have long been known to be mixed with the straight hair of various animals, such as the "deer, hare, rabbit, beaver, otter, seal, lion, tiger, certain varieties of dog, and some foreign breeds of oxen." All these hairs are so much alike, structurally, that it is believed they should all be designated as wool, and it is not claimed that the animal from which they were derived can be uniformly and reliably determined by the microscope. Obviously some of these varieties not now recognized as wool might in the future become of sufficient commercial importance to require either the legalization of them all as "wool," or the discovery of more complete methods of discrimination. Second, straight hairs. These are often shorter, "much thicker at their base, and taper rapidly towards the point. The medulla occupies a large proportional part of the whole hair, and the free edges of the scales of the cuticle, which are so disposed as to form from twenty to forty imbrications to the $\frac{1}{100}$ of an inch, lie quite smoothly upon the surface of the hairs, so that their contours, as seen under the microscope, closely approximate continuous lines. These characters are so well marked that the coarser hairs of the cow and calf can readily be distinguished from the woolly hairs of any of the wool-bearing animals." Naturally mixed with the wool of the sheep, however, especially with the inferior grades, and with that of the goat, forming the "outer coat" of the goat, are coarse, straight hairs, so closely resembling some of the hairs of the cow or calf that their discrimination presents great difficulties; and such hairs, even when derived from the wool-bearing animals, cannot be recognized as wool by the microscope. The percentage of "wool," therefore, as determined in mixed fabrics, by a microscopical count of hairs, would probably be underrated in a certain proportion of cases. In case all woolly hairs which are "more or less crispy, curled, or frizzled," should be legalized as wool, it would probably be convenient to make an exception, ad-

mitting as genuine wool such a percentage of straight hairs as is found to be present in a specified quality of the sheep's coat. Third, doubtful hairs. Among the imbricated hairs of the wool of the sheep some are occasionally found which so closely resemble the softer hairs of the cow or calf that the investigators confess themselves unable to discriminate between them in all instances. Hairs of this description are therefore more properly classed as doubtful, than included in either of the other groups.

Microscopy at the American Association.—The 'American Naturalist' (October) says that the subsection of microscopy of the American Association for the Advancement of Science, which has hitherto been a transient organization, temporarily formed whenever necessary, was established as a permanent body at the Buffalo meeting in August last. In addition to business connected with the details of organization, nine papers were read, and many interesting discussions were held. Two evenings were occupied, one by an informal soirée at the rooms of the subsection, and the other by a very successful reception tendered by the Buffalo Microscopical Club. The members present were cordially and unanimously in favour of the permanent organization. Dr. R. H. Ward, of Troy, was elected chairman for the first year, ending with the Nashville meeting next August.

An Objection to the term Microscopy, which had been raised some time since in a paper published in 'Science Gossip,' by Mr. F. Kitton, has called out the criticism of an American writer, who says that "Mr. Kitton objects strenuously to the terms microscopy and microscopist. He argues that there is no such science as microscopy, because its objects of study belong to zoology, botany, &c. Precisely the same statement might be made in regard to anatomy, physiology, and to histology, which he mentions in the same sentence without protest. All of these terms are too convenient to die, and the 'microscopy' of 'Science Gossip' itself is too good to be suppressed or dispersed by suicidal theories in regard to its name. Even as a word, microscopist is no worse than pianist or organist, and microscopy is as good as thermometry."

CORRESPONDENCE.

Dr. Abbe's Letter on Dr. Pigott's Paper.

To the Editor of the 'Monthly Microscopical Journal.'

Petersfield House, Cambridge, *November* 4, 1876.

Sir,—If, according to Dr. Abbe's letter in your last number, I have inadvertently failed to note priority, and to give sufficient prominence, honour, and credit to that gentleman's result, no one more regrets it than myself. Writing from memory, and not having read Dr. Abbe's paper except in extracts without the formula, my opinion was formed from Professor Helmholtz's own statement as translated by Dr. Fripp.

"That diffraction and consequent obscurity of microscopic image must necessarily increase with increasing amplifications of the image, and this, quite independently of any particular construction of the instrument, rests as a fact upon a general law which applies to all optical instruments, and which was first formularized by La Grange for combinations of any kind of infinitely thin lenses. The law has apparently remained unknown, perhaps, says Helmholtz, because La Grange enunciated it in equations (formulæ) whose coefficients have not characters which readily present clear ideas to the mind. In my treatise on physiological optics I have given expression to this law in a somewhat more general form . . . and have endeavoured to formularize it in readily intelligible characters." Professor Helmholtz here states he has endeavoured to formularize these complicated mathematical expressions so as to be readily intelligible. This, in plain English, I have expressed "by popularized." And I trust this will give no offence to this distinguished and highly gifted savant. Dr. Abbe, perhaps, would do me the honour to send a copy of his paper to me. But whether it would receive any damage by an attempt on my part to *popularize* his physical reasoning by which he arrived at the result (afterwards given by Helmholtz) omitted by Dr. Fripp, I must leave him to judge for himself.

I am, Sir, your obedient servant,

G. W. Royston-Pigott.

N.B.—The primary object of connecting mathematical representations of waves with acknowledged dynamical principles formed the basis of the investigations of Huyghens, Euler, and D'Alembert (Berlin Acts, 1749), and by La Grange ('Turin Memoirs,' 1759), and it was soon seen to apply to the case of undulating ether; a long list of worthies have devoted their resplendent talents to the question of the wave theory and its phenomena. Our own savant, Sir J. F. Herschel, expressly states in his work on 'Light,' "that Fraunhofer seems inclined to conclude that an object of less linear diameter than λ (a wave-length) can never be discerned by microscopes as consisting of parts: a conclusion," says Herschel, "which would put a natural limit to the magnifying power of microscopes, but which we cannot regard as following from the premmises."

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *November* 1, 1876.

H. C. Sorby, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of presents to the Society was read, and the thanks of the meeting were voted to the donors.

The Secretary called the particular attention of the Fellows to a

very handsome work "On the Quinology of the East Indian Plantations," presented to the Society by the author, John Elliot Howard, Esq.. F.R.S., and a special vote of thanks was recorded.

The President reminded the Fellows that they would hold a scientific meeting for the examination of objects, &c., on Nov. 8, and expressed a hope that there would be an effort made to render the occasion as interesting as possible.

A paper by Dr. G. W. Royston-Pigott, "On a New Refractometer for Measuring the Refractive Index of Thin Plates of Glass, Tubes, &c.," was read by the President, and was illustrated by diagrams enlarged upon the black-board, and also by the exhibition of the instrument itself, specially lent for the occasion from the Loan Collection of Scientific Apparatus at South Kensington.

The President thought the plan adopted by Dr. Pigott was very ingenious, and would in many cases prove to be very useful where no other means could be adopted, as, for example, in identifying the minerals seen in thin sections of rocks. He might mention that he had been lately studying the microscopical characters of various sand deposits, and had found amongst the grains a material which had a much higher refractive index than that of quartz. When mounted in Canada balsam, and illuminated by a spotted lens, pure quartz, owing to its very similar refractive index, was almost entirely lost to sight; but he had found in some sand from the neighbourhood of Dunkeld grains of a substance which reflected brilliant light, owing to its much higher refractive index, and he thought it probable that this might prove to be corundum, which he believed had not hitherto been recognized as forming any part of our native sands. In such cases as this he thought the apparatus described by Dr. Pigott might be very useful.

A vote of thanks to Dr. Pigott for his communication was unanimously carried.

A paper by the Rev. W. H. Dallinger, "On some Experiments with a Sterile Putrescible Fluid exposed in an Optically Pure Atmosphere, &c.," was read by Mr. Charles Stewart, who drew upon the black-board a sketch of the apparatus described and figured by the author, and explained the means which had been adopted in carrying on the experiments. (The paper will be found at p. 288.)

The President, in proposing a vote of thanks to the author of the paper, felt sure that all present must greatly admire the industry of Mr. Dallinger, as well as the ingenious contrivance which he had described; and they would no doubt be interested in the curious results obtained. Having never himself made any experiments on these subjects, it would hardly become him to do more than call attention to the importance of these investigations as bearing upon those questions of the relations between life and matter which had given rise to so much discussion of late.

The thanks of the meeting were unanimously voted to the Rev. W. H. Dallinger for his paper.

Mr. Charles Brooke inquired if any time was stated as being required for the deposition of the germs of the monads?

Mr. Stewart said that it appeared from the paper that after the first introduction of the powder four and a half hours were allowed for the subsidence of the coarser particles, and then it was left for twenty-four hours; after this the covers were removed and the vessels left for four days before their contents were examined.

The President said that his attention had been drawn to analogous phenomena, and he had found that although the rate at which particles of a substance subsided is in proportion to their size, yet the relative positions of the particles at the time their subsidence took place must also be taken into consideration.

Dr. Lawson inquired to what temperature the dried-up portion of cod's head had been submitted?

Mr. Stewart said it was 150° Fahr., a temperature which it had been previously proved was sufficient to kill the adult forms of the monads. He thought it quite probable upon the face of it, that the germs derived from the larger forms would be in themselves larger than those of the small forms, and therefore it might be inferred that they would probably subside first.

Dr. Lawson remembered that in cases where *Rotifera* had been subjected to an exceedingly high temperature, they had survived in those instances in which the heat had been gradually applied.

The President said that the temperature which had been mentioned was that which coagulates albumen, and might thus destroy life for which albumen is necessary, but not in those cases in which it is unnecessary for life.

Mr. Stewart did not think it followed necessarily that the creatures themselves had been exposed to this temperature, as they might have been protected by some surrounding covering.

A paper by Mr. Wenham, "On the Measurement of the Angle of Aperture of Object-glasses," was read by Mr. J. E. Ingpen, and illustrated by drawings on the black-board enlarged from the diagrams accompanying the paper. It will be found at p. 285.

A vote of thanks to Mr. Wenham was unanimously passed.

The President said that his attention had lately been drawn a good deal to this subject, and he had found that in many cases the working aperture was much less than the aperture which had been stated as being that of the lens. He then proceeded to show, by means of diagrams drawn upon the board, a method which he had employed for ascertaining the angles at which rays could be sent upon an object, so that it might remain dark upon a bright background, the light being reflected upon it by means of a prism having angles of 55°, 60°, and 65°.

Mr. Charles Brooke suggested another means of throwing a parallel pencil of light upon the object, by employing a tube having a concave lens at one end and a convex lens at the other.

The President recognized the merit of this plan, but suggested that it was open to the objection of not being so readily illuminated by means of the ordinary mirror.

Scientific Evening, November 8, 1876.

On Wednesday evening, November 8, a conversazione was given by the Royal Microscopical Society, the library of King's College having been kindly placed at its disposal by the authorities. In every sense of the word the evening was a complete success, the objects exhibited being at once interesting and rare, and, as will be seen by the annexed list, being both numerous and varied. There were about a hundred present, and there was hence abundance of room, and everybody was enabled to examine the different specimens exhibited with ease and comfort.

The thanks of the Society are especially due to Mr. How and to Mr. Charles Baker for the loan of the lamps, which were a great acquisition to the exhibitors of objects. Among the special things exhibited by the Society may be mentioned the Quekett Medal, which it is proposed to bestow on certain lecturers selected by the Council of the Society, and which really looked remarkably well.

Tea and coffee were served in the course of the evening, and altogether the gathering may be said to have been one of the most successful that have been held at King's College.

Objects Exhibited.

Messrs. Beck: New form of achromatic condenser, and crystallized gold, &c.

Mr. John Browning: Sorby's improved micro-spectroscope, with new measuring apparatus; the McLean star spectroscope, requiring no slit and used like an ordinary eye-piece; also table polariscope.

Mr. Brindley: Sections of boulders from the glacial clay.

Mr. Baker: Bramhall's illuminator.

Mr. T. Curties: Seminal glands of flea.

Mr. W. Cocks: *Lacinularia socialis* and *Carchesium polypinum.*

Mr. F. Enock: *Stylops Spencii* emerging from the abdomen of bee and various insects, mounted without pressure.

Mr. Frederick Fitch: Reproductive organs of the wasp.

Rev. T. W. Freckelton: New arrangement for attaching the polariscope and analyzer to the microscope, so that both can be put instantaneously out of the field without unscrewing.

Mr. A. de Souza Guimaraens: Sections of pearls in different stages of development.

Mr. A. Hilger: A new form of direct vision, very powerful spectroscope, dividing widely D with nickel line between when on sun, with micrometer arrangements to measure relative positions to 0·0001 to the inch. A new micrometer for having two spectra at the same time, or even THREE, and measuring to 0·00020 part of an inch intervals between lines. A new pocket spectroscope as devised by the President, with unusual power for the small size. Diverse eye-pieces, with arrangement to measure faint stars.

Messrs. How and Co.: Sections of dolerite, &c.

Mr. Thomas Howse: Crystalloids in albumen of nutmeg.

Mr. John E. Ingpen: Proposed general standard of magnifying power. The microscope is arranged as in ordinary use, and focussed

on an object, below which is placed an illuminated circular disk at a distance of ten times its diameter. The power is in the inverse proportion of the diameter of the disk to its diminished image in the eye-piece.

EXAMPLE.

Diameter of disk	·25
Distance from focus	2·5
Diameter of diminished image	·0182

$$1 \div \cdot 0182 = 54 \cdot 94$$

The power required.

A slit or divided scale may be substituted for the disk, provided that it be placed at a distance of ten times its unit of length from the focus.

The disk or scale can be measured by a double-image dynameter, or by a microscope with a Jackson or other micrometer.

Dr. Lawson: Human cerebellum and spinal cord of cod.

Mr. Lettsom: Professor Noerremberg's wide-field polarizing microscope, and Brookite, chrysoberyl, corundum var. ruby, parisite, platinocyanide of yttrium, Smithsonite, and titanite.

Mr. Moginie: Diatoms from Richmond, U.S. (Systephania).

Mr. McIntire: Curious butterfly scales, species?

Dr. Millar: Sections of new sponges, *Dactylocalyx polydiscus* and *Acanthospongia Smithii.*

Mr. Norman: Crystals of gold and sporules of fungi.

Mr. Palmer: New micro-spectroscope apparatus for measuring the position of bands, with illustrative specimens including solution of pyrethrum in ether.

Mr. Walter W. Reeves: Fructification of Chara, *Chara fœtida*, with globules containing the spiral filaments; and *Chara fragilis?* with the spiral filaments squeezed out of the globules.

Mr. James Smith: Elytron of beetle, *Chrysolophulus spectabilis.*

Mr. H. J. Slack: "Brighton pebble," with red round hollow bodies, perhaps fossil (?).

Mr. J. H. Steward: Fire-tailed bee.

Mr. Sigsworth: Ovaries of various plants, with ovules *in situ.*

Mr. Charles Stewart: Pedicellaria of one of the echinoderms.

Mr. H. C. Sorby: Red clay and other deposits from great depths in the Pacific Ocean, from the 'Challenger' expedition. Various specimens show that the red clay contains grains of sand as well as volcanic products, and is in many respects analogous to the gault and other fine-grained deposits. Also various colouring matters derived from bile occurring in vomit, with other specimens illustrating their nature and their connection with those found in normal feces and urine. The part soluble in ether gives a spectrum quite unlike that of the analogous substance from the bile of the ox or sheep. That soluble in water contains the same colouring matter as that found in normal feces, and another apparently identical with a product obtained by the oxidization of the bile pigment present in the urine of certain cases of jaundice, and analogous to a constituent giving a well-marked spectrum, present in variable quantity in normal urine.

Mr. Topping: Section of the head of lamprey.

Mr. Ward with Mr. Bevington: Raspberry seed, and sections of various charred woods from the lake dwellings of Rohenhausen.

Donations to the Library and Cabinet since October 4, 1876:

	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*Society.*
Journal of the Linnean Society. No. 86	*Ditto.*
American Journal of Microscopy. No. 10	*Editor.*
Bulletin de la Société Botanique de France	*Society.*
Quinology of the East Indian Plantations. By John E. Howard. 1869 and 1876	*Author.*
Proceedings of the Literary and Philosophical Society of Manchester. Vol. XV.	*Society.*
Memoirs of ditto ditto. Vol. V.	*Ditto.*
Four Slides of Sections of Coal Fossils	*Mr. Norman.*

The Rev. John Spaven, of Windermere, and Dr. William Morris, of Sydney, N.S.W., were elected Fellows of the Society.

WALTER W. REEVES,
Assist.-Secretary.

MEDICAL MICROSCOPICAL SOCIETY.

Friday, October 20, 1876.—F. T. Payne, Esq., President, in the chair.

Superficial Gangrene of Skin.—A specimen of this was exhibited under the microscope by Mr. Golding-Bird, and he pointed out that the section having been taken at the junction of the black or truly gangrenous part with the red, inflamed skin beyond, the difference between the two states was histologically, that in the former the papillæ were partially or entirely destroyed, and the epidermis was more completely separated from the corium than in the latter, where the papillæ remained and the earliest formation of a vesicle was well seen; the cuticle was separated in isolated patches by serum that was now distinctly visible, having been coagulated by the reagents used. Throughout the specimen inflammatory cell infiltration was plainly seen.

Dr. Pritchard exhibited and explained an ingenious form of microscope by which the circulation of the blood in the frænum of the human tongue could be watched. The essentials of the instrument were a tube carrying an ocular and a No. 2 (Hartnack) objective, and to the end of the latter was fitted a brass cap, from which a hollow rod of metal, of the size of a crowquill and about an inch long, projected. The cap was centrally perforated. The narrow end of an ordinary ear speculum was presented to the extremity of this small tube and held adjacent to it by a wire clip, and so arranged that microscope tube, metallic rod, and ear speculum had each its axis in the same straight line. To use it the frænum linguæ was placed between the hollow rod and the speculum (this latter condensing the light), and when focussed the circulation could be watched.

Diffuse Cancer of the Liver.—Dr. Goodhart showed specimens taken from a child æt. 10, who had during life all the symptoms of

a cirrhosed liver, but a post-mortem examination revealed diffuse carcinoma of the organ.

Epulis-Myeloid Sarcoma.—Mr. Needham, in exhibiting a section of the growth, remarked that he had brought it as being the first time he had ever seen myeloid (giant) cells in an epulis. He had examined about three dozen in all, but always had found them fibrous in character. This tumour had been growing but ten days; the patient was a child who had had a tooth knocked out by accident, from the socket of which at once the epulis began to appear.

A discussion ensued, and the President and Dr. Goodhart expressed their opinion that the giant-celled epulis was found far more frequently than Mr. Needham had stated.

Spinal Cord in Leprosy.—Dr. Pritchard had found in specimens of the above, peculiar rounded microscopic bodies scattered throughout the nerve substance. As far as he knew the case was one of tubercular, not anæsthetic, leprosy.

Freezing Microtome.—Mr. Williams showed in action a microtome invented by himself, but first suggested by the simple but effective microtome of Dr. Pritchard. The instrument consists of a wooden block, hollowed out and having rising from its centre a rod of brass, terminating in a flattened head the size of a penny-piece. This projects slightly through a central aperture in piece of plate glass, which acts as a lid, and is strongly set in a wooden frame. The hollow block being filled with pounded ice and salt, the cover is fitted on, and the upper surface of the central brass stem smeared with mucilage. As soon as the mucilage whitens, indicating that the brass support has reached freezing point, the piece of tissue, animal or vegetable, is placed upon it and covered over. It sets firmly almost at once, and then the section is cut from it at one sweep by a straight-edged razor set in a triangular brass frame, which is rapidly pushed over the surface of the glass lid; the height of the razor, and therefore the thickness of the section, being regulated by three screws which support the triangle, and on the points of which it is slid backwards and forwards. The essential difference between this microtome and most others is in this last-mentioned particular, i. e. that the specimen remaining stationary, the razor is lowered or raised to meet the requirements of the section. By means of it Mr. Williams had successfully cut from a leaf, first a complete section from apex to base, including just the cuticle, and then a second, including the parenchyma; so accurately can the razor be adjusted. The ice being in a wooden receptacle, remains in action for a long time.

Quekett Microscopical Club.

Ordinary Meeting, September 22, 1876.—Henry Lee, Esq., F.L.S., President, in the chair.

A paper by Mr. W. K. Bridgman, "On an Improved Anti-vibration Turn-tray," was read. The principle upon which this instrument was constructed consisted in opposing two forces in opposite directions. If a board be supported upon four bent springs,

one at each corner, these, when weighted, become extended, and thus shift their points of contact with the support, producing friction, which tends rather to promote vibration than to absorb it; but if these springs be made to rest upon and be attached to four other similar springs in an inverted position, any upward tremor communicated to the lower set will be taken up and destroyed by the upper set, and so leave the top board comparatively at rest. In the instrument described india-rubber rings were employed, the compression of one set of rings being counterbalanced by another set extended over the corners, which were notched out to receive them: other springs being inserted at intermediate points to secure greater stiffness of action against lateral movement. The two boards being thus in contact only through the medium of the rings all upward tremor from passing traffic was effectually destroyed, and the microscope and lamp being supported by the turn-tray could be readily passed from one observer to another without any alteration of position.

Ordinary Meeting, October 27, 1876.—Henry Lee, Esq., F.L.S., President, in the chair.

Mr. T. C. White read a paper upon the Fly Fungus, *Empusa musci*, in which he detailed its appearance and peculiarities, illustrating the subject by specimens exhibited under the microscope.

Mr. W. K. Bridgman read a paper "On a New Universal Reflecting Condenser." The design of this arrangement was to acquire a control over the angle of illumination, so as to be able to direct the light readily at any degree of obliquity from a dark ground to axial rays, and thus not only to secure the proper angle for every object and any objective, however wide-angled, but also to be applicable to all other purposes. A small metal speculum was fixed vertically to a cross-bar, which also carried a second reflector acting as a stop or diaphragm, and opened and shut by a screw so as to regulate the quantity of light admitted. The light thus passing through a narrow slit, and being reduced almost to the same plane, was deprived of all cross rays tending to interfere with distinct vision, and as the reflector and its stop could be moved backwards and forwards, and also inclined at any angle by a simple finger and thumb movement, it admitted of being adjusted with the greatest nicety. By these means the angle of illumination could be gradually changed until the most suitable effect was produced—a certain obliquity causing light to be reflected down upon the object from the covering glass, giving the effect of surface illumination in addition to transparency, and bringing out surface markings in a manner not otherwise so easily attainable.

Mr. W. H. Gilburt read a paper "On the Relations of *Volvox globator* to *Sphœrosira volvox*," showing that the last-named form, which had been classed as a distinct species by Ehrenberg, was, as had been suggested by Mr. Busk and others, only a stage in the life history of the better-known *V. globator*. The writer had found a large number of *V. globator*, containing one or more *Sphœrosira*, together with other macro-gonidia, it being a very rare exception to find more than one. He also described the compound discoid bodies found in *Sphœrosira*, and certain differences observed in the other

macro-gonidia contained in the same mother-cell with *Sphœrosira*, which, taken together, seemed to suggest a sexual relation between them. Specimens of *Volvox globator* containing *Sphœrosira* were exhibited at the meeting.

Microscopical Society of Liverpool.

The seventh ordinary meeting of the eighth session of this Society was held on Friday evening, October 6, at the Royal Institution, the Rev. H. H. Higgins, M.A., President, in the chair.

The Honorary Secretary announced an unusual number of donations to the library and cabinet of slides, after which, in accordance with Rule 3, the election of President for the year 1877 was proceeded with, the Rev. H. H. Higgins being re-elected by acclamation. Colonel Woodward, United States Army Medical Museum, Washington, U.S.A.; Professor Thomas Taylor, chief of the microscopical staff in the Agricultural Department, Washington, U.S.A.; and W. G. Corthell, Esq., 103, Warren Avenue, Boston, U.S.A., were elected honorary members of the Society.

Mr. Isaac C. Thompson gave a short account of some of the eminent microscopists of the United States he had met with during a recent visit to that country. He described the process of microphotography, adapted so successfully by Colonel Woodward, head of the Army Medical Department of the United States, at Washington. By using the heliostat, from whence was thrown a fine pencil of sunlight through the microscope into the camera in a darkened room, Colonel Woodward prepares the finest enlarged micro-photographs ever produced. He had, among other things, by this means clearly demonstrated that any difference between the blood of man, dog, guinea-pig, is microscopically indistinguishable, a fact which had been previously contested. Mr. Thompson spoke of the microscopical department at the Centennial Exhibition, stating that our well-known English makers, Messrs. Ross and Co., R. J. Beck, and Crouch, were prominent exhibitors. The Americans were represented by Messrs. Zentmeyer, and M'Queen and Co.; and though Americans are behind us in microscopes and apparatus generally, they are very skilful as working microscopists. He was glad to hand over to the Society's cabinet some valuable slides of double-stained vegetable sections, on behalf of Mr. Corthell, of Boston, the donor and mounter. Mr. Thompson also exhibited some eyeless fish he had brought from the Mammoth Cave in Kentucky, and concluded by expressing a belief that the American microscopists, Colonel Woodward and Mr. Corthell, whose names he had the pleasure of proposing as honorary members, would be a most valuable and helpful acquisition to the Society, and a means of co-operation in microscopical research between the two countries.

The President, as the proposer of Professor Taylor, said he had known him personally for some years, and had recently seen him at Washington. He spoke in very high terms of his labours as a microscopist, and said many of his communications were to be found in the American Government reports on agriculture.

Mr. Chantrell, Honorary Secretary, drew attention to an elaborate paper, well illustrated by Professor Taylor, "On Fungoid Diseases of Plants," which appeared in the Annual Report of the Commissioner of Agriculture for the year 1871.

The President exhibited and described a number of lichens and mosses which he had collected during his recent scientific expedition in the 'Argo,' many of which were extremely rare, and several new to science.

The meeting concluded with the usual conversazione. The exhibition of microscopes being unusually large, there was a good attendance of members and visitors.

San Francisco Microscopical Society.*

A regular meeting of this Society was held at its rooms in California Street last Thursday evening, Vice-President H. C. Hyde in the chair.

Mr. Moore exhibited a section of the trunk of a pine of a very peculiar variety, whose history he will fully elucidate at the next meeting. Dr. Wythe exhibited a sertularian polyp from Monterey Bay, covered with diatoms of various sorts, navicular and sertulariæ, &c. The specimen was interesting from its novelty, and the question arises as to whether the polyp was the habitat of the diatoms or the latter an accidental accretion.

Fourteen Rock Sections from the Comstock Mines, by Melville Atwood.—The slides marked Nos. 1, 2, and 3, are sections of rocks from what, at the Gold Hill portion of the Comstock, is known as the Black Dyke. The specimens from which they are cut were taken from the Dyke at different depths below the surface, at 1100, 1400, and 1740 feet. I have made a careful examination of them, and think they are from a basaltic rock belonging to the class called Dolerites, in confirmation of which, these sections will be seen to consist of crystals of Labradorite and Augite, imbedded in a greyish paste or matrix, in which are also numerous minute particles of magnetic iron—presenting that mottled appearance so characteristic of that class of volcanic rocks.

Slide No. 4 is a section of Dolerite and Anamesite, from Bolvershahn, in Siebengebirge. It resembles the Black Dyke so much that I can hardly distinguish any difference between them.

Slides No. 5 and 6 are sections from the unaltered trachytic greenstone. Specific gravity 2·7. Contains silica 52 p. c.

Slides No. 7 and 8 are sections of the altered trachytic greenstone. The green mineral so conspicuous in these altered rocks is smaragdite. Slides Nos. 9 and 10 are sections from what may be called "True Horse," found in the ore bodies.

Slides Nos. 11 and 12 are sections from the Comstock ore, showing how the argentiferous gold is mechanically mixed with the silver ore.

* [Unfortunately the Secretary sends us these Reports without any date whatever.—Ed. 'M. M. J.']

Slides Nos. 13 and 14 are sections cut from rock taken from different points on Mount Davidson and sent to me as specimens of the Mount Davidson syenite. On examination it will be seen that the feldspar is not *orthoclase*, so that it is a diorite, instead of a syenite. I will try and procure some more specimens and submit them to the Society at some future meeting.

The regular meeting of the San Francisco Microscopical Society was held on Thursday evening last, with Vice-President H. C. Hyde in the chair.

Dr. J. H. Wythe donated two slides mounted with *A. Ehrenbergii* and *I. nervosa* as opaque objects, which were obtained from Monterey Bay.

Mr. J. P. Moore presented the Society with the fungoid growth which he described at the last meeting of the Society, and named *Agaricus tridens.* He made a statement to the effect that when the mycelium first begins to make its appearance on the timbers in the drifts of the mines, it presents a pure white appearance, and seems to burst out of the wood. It is called by the miners, the Lily of the Mines.

Mr. H. G. Hanks presented some specimens of gold on crystals of pyrites, which seemed to have been squeezed from the pyrites. He also presented a sample of pure ground coffee and another of the ground articles of commerce, which a microscopic examination by him showed to be nearly 50 per cent. chicory.

Mr. Henry Edwards presented a quantity of diatomaceous earth, from near Los Angeles, containing many interesting fossil forms of the beautiful silicious frustules ; and from the bottom of Lake Tahoe, 600 feet below the surface, came similar forms to bear them company, obtained by Dr. Blake, on a recent visit to that locality. Dr. Blake also exhibited two varieties of entomostraca, found in myriads in the surface waters of the same lake, but which were passed over for the present with a cursory examination.

Mr. J. P. Moore exhibited a super-stage, which he had caused to be made from a model described in the 'Quekett Journal of Microscopy,' the advantages of which he stated at some length, and which he had verified recently.

Dr. J. H. Wythe brought to the rooms his Crouch binocular instrument, for the purpose of exhibiting some further improvements made in his oblique condenser and amplifier. His tests with a low power over *P. angulatum* and *S. gemma* for magnification and definition at the same time, were conclusive as to the merits of both accessories. Dr. Wythe stated to the members present that he had taken the liberty of dedicating his forthcoming work on microscopy to the San Francisco Microscopical Society, and that the volume was in the press, and would soon be out.

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